urging the benefits of temperance. A desire to avoid susceptibility of the disease is one of the strongest motives by which to influence men's minds; already, indeed, has this salutary principle begun the work of reformation; and it is engendering habits of regularity, which the unaided precepts of prudence or morality have never succeeded in effecting. In France they have too much sense to keep the people in ignorance, lest they should be frightened; and it is ordered, that every house in Paris be visited by persons competent to judge of its condition as to ventilation, sewers, and other circumstances of a similar nature, so that not a family in that great metropolis but have the probable approach of the disease forced upon their attention; and we anticipate as the result, that all which human sagacity can devise will be put in operation to prepare for its reception. Why should it not be the same with us?—why are not the civil authorities enjoined to enforce the necessary precautions? Let us be prepared, if the disease should unhappily reach us, to meet the emergency with calm and well-directed efforts; and above all, let us not be betrayed into a false security by those who make light of a disease in which they have no experience. Those who talk the boldest while the enemy is not within sight, are generally the first to play the craven when he comes. Are they who exclaim that the alarm is false aware that it is ascertained that not fewer than an hundred thousand persons have been cut off in Hungary, and eighty thousand in Galatia, where the principles of non-contagion have prevailed, and where the people have waited for the disease in passive inactivity? Should the confident predictions with which the press has been teeming, that the disease will either not come, or come robbed of its terrors, prove fallacious, we should then have to encounter the most fearful of all calamities in such a case—that of a sudden and general panic, by which the energies of the public mind would be paralyzed at the very moment when coolness and self-possession would be most required.

Since writing the above, our attention has been directed to some observations of Dr. James Johnson, recently published in the Times; we have also seen another letter from that gentleman in the Herald, into which it had been copied from the Courier; and we perceive that we are to be favoured, through the same medium, with a succession of papers of a similar nature. This somewhat singular mode of giving publicity to his opinions, the worthy doctor assures us, does not proceed either from "vanity or self-interest," and that he has no end in view but "the good of his country." That he should anticipate such a result from his epistles puts at rest the idea of "vanity" having any share in his motives. With regard to the absolute absence of "self-interest" we should not have been so well convinced had not the doctor given us the above satisfactory assurance; but for this, it is just possible, that having his name and address kept before the public in successive numbers of a daily paper in extensive circulation, and accompanied by the laudatory comments of a friendly editor, might have been regarded by ill-natured persons as having some slight weight in leading him to adopt a proceeding so unusual with respectable members of our profession. We subjoin an extract from the Courier, that our readers may judge of the tone in which the passages we allude to are written:—

"Dr. Johnson is, from his long acquaintance with the disease in India, and from his high standing here as a physician, a better authority, perhaps,

than any other in this country; and it is not very creditable to the Board of Health to have put forth their opinions publicly, without having consulted that gentleman.'

We cannot for a moment suppose that Dr. Johnson had any thing to do with the insertion of this, or other equally absurd paragraphs; but we do think that most men would have avoided affording them even the indirect countenance they receive from his immediately becoming a contributor to the paper in which they appeared.

EXTRACTS FROM JOURNALS,

Foreign and Domestic.

ON THE IDENTITY OF SMALL-POX AND COW-POX, AND ON A MODE OF INDUCING THE VACCINE PUSTULE IN THE COW AT PLEASURE.

Dr. Sonderland, of Bermen, the author of the paper which we shall here translate almost without abridgment, if his experiments be correct, has at length succeeded in establishing what physicians have long laboured to discover—a satisfactory and simple explanation of the protective power of cow-pox against small-pox; and has announced, we will venture to say, the most important discovery which has been made in the pathology of these diseases since vaccination was first introduced—by shewing that they are modifications of one another, and that cow-pox in the cow is simply small-pox in man, and may be produced in that animal at will by the variolous contagion. Of the authenticity of his facts we do not pretend to judge. All we can say is, that the author, if we judge from the language of Hufeland towards him, is a respectable practitioner, and a public medical officer.

"The simplest and surest mode," says he, "of producing cow-pox in the cow, and thus proving indisputably the identity between the contagion of cow-pox and that of human small-pox, is to follow the procedure here laid down.

"Take a woollen bedcover which has lain on the bed of a small-pox patient who has died during the suppurating stage, or is suffering from the disease in a considerable degree, and is lying in a small, imperfectly ventilated apartment; and when it is well penetrated by the contagion, roll it up immediately after death or on the fourteenth day of the disease, wrap it in a linen cloth, and then spread it for twenty-four hours on the back of a quey in such a manner that it cannot be thrown off by the animal. Then place it for twenty-four hours on the back of each of three other queys, and afterwards hang it in such a manner in their stall that its exhalations may rise upwards and be inhaled by them. In a few days the animals will fall sick and be seized with fever; and on the fourth or fifth day the udders and other parts covered with hard skin will present an eruption of pustules, which assume the well-known appearance of cow-pox and become filled with lymph. This lymph, which exactly resembles the lymph of genuine cow-pox, if used for inoculating the human subject, will induce the vaccine or protective pock. The only precaution which it is necessary to observe is that the person about to be inoculated shall not be exposed in any manner to the contagious effluvia of the cow-house either directly or through the intervention of the experimentalist's clothes, otherwise he may have natural small-pox.

"A bedcover impregnated with the variolous contagion, if firmly rolled up and wrapped in linen, and afterwards in paper, and then properly packed in a bucket, will retain the contagion for at least two years, so as to infect a cow with cow-pox, provided it be kept in a cool, shady place, where the temperature does not fall under 32 or above 52 degrees.

"My present occupations prevent me at this particular period from giving a full and scientific exposition of the consequences which must follow from this discovery; but I may state them shortly in the aphoristic form.

1. "This discovery is new; for, although many have suspected the identity of small-pox in man and cow-pox in the cow, and have in consequence performed inoculation with the matter of both, yet no one has previously ascer-

tained the possibility of transmitting the contagion to the cow in the gaseous form, so as to decide the question beyond all doubt.

2. "The desire of physicians and governments to discover cow-pox in cows, in order to revive the vaccine lymph, is more than fulfilled by the discovery of a simple method of engendering cow-pox in the cow at will.

3. "Jenner's discovery of the protective power of vaccination hitherto imperfect, is now perfected, because the hitherto unknown nature and origin of cow-pox are laid open.

4. "All previous uncertainty regarding the quality of vaccine matter, its degeneration, the loss of its protective property, and the like, must now cease, because we have obtained a clear insight into the nature of cow-pox and can lay down a substantial theory of its operation.

5. "This discovery must tend to widen the boundaries of physiology, pathology, and therapeutics, since it shews how the subtile contagion of small-pox, so hostile to the nervous system of man, may be conveyed in the aeriform state from him to the cow, excite in that animal a similar disease, but in doing so be changed by the special constitution of this class of animals into a permanent contagion of a different kind.

6. "An instructive lesson may be drawn from this discovery how the poison of diseases in the gaseous form may be communicated to the lower animals, and according to the difference in their constitution engender diversified products, which may be then used as protective means against the diseases from which they originated. Such, for example, may be subsequently proved of scarlet fever, measles, yellow fever, and plague.

7. "It is now clear why, in recent times, cow-pox has been seldom or never seen in the cow. For the cow-pox of the cow arises merely from infection by the variolous exhalations from men recently affected with small-pox, and coming in contact with the cow. As epidemics of small-pox have been rare during the last thirty years, cows could seldom be exposed to infection, and have therefore seldom exhibited the disease."—*Edin. Med. Journ. and Journal des Praktischen Heilkunde, Januar.* 1831.

BRONCHOTOMY IN THE HORSE.

The number of the Veterinarian for November contains four cases in which the trachea was opened, with the effect of saving the lives of the animals: these were all instances in which suffocation was impending from inflammation about the larynx. The following is the description of the operation as performed in the first case:—"An incision was made, about four inches in length, through the integument and sterno-thyroideus muscle; a portion of one of the rings of the trachea was excised, large enough to admit one's finger; a thread was passed through the muscle and integument on each side, and fastened to the mane, to prevent the aperture from closing." The cases were treated by Mr. Toombs, of Great Barrington.

WESTMINSTER MEDICAL SOCIETY.

Saturday, Oct. 29th.

The Cholera.

A REPORT having been circulated through the newspapers, that the all-engrossing subject of cholera was to be discussed here this evening, there was a very full attendance of members and visitors.

DR. SIGMOND in the Chair.

Whether the "order of the day" had been correctly reported or not, we cannot tell; but certain it is, that much time was lost in solemn silence after the president had announced that the business might now begin, until at length

DR. GRANVILLE volunteered to open the discussion. He expressed himself highly dissatisfied with the regulations of the Board of Health, which he thought were both useless and severe. On other occasions he had proved himself a stout advocate of contagion—as all would remember, on the Plague question; but he would now take credit to himself for maintaining as strenuously the opposite doctrine with regard to Cholera: on this question he was a steady non-contagionist. There was one great character of a contagious disease absent in cholera, there was *no eruption*. Nor was the spreading of the disease like that which would result from infection or contact: in St. Petersburgh, for instance, where the utmost care had been taken to exclude the malady, *one* case was at length officially

announced, and in less than four days after there were between 400 and 500. How could this possibly have resulted from contagion, unless the sick had actually run out into the street, and touched and infected every one they met? The same conclusion was also to be deduced from the history of the disease, as it appeared in other places. He regretted that we had not sufficiently exact accounts of the pathology of cholera: of the matter or fluid discharged from the stomach, there was but little information: it was only occasionally, and almost by accident, he (Dr. G.) could gather that it was sour to the taste; from which he was naturally induced to infer the presence of muriatic acid, especially as the other symptoms would warrant the supposition, that the patients suffered as if poisoned with that acid. The learned Doctor having indulged, in the course of his speech, in a vein of merriment upon the published regulations of the Board of Health, and upon the history of the disease generally, as given by the contagionists, apologized, in conclusion, for having treated with levity a subject of such awful interest: he did it merely, he said, to give animation to his arguments.

Dr. O'Shaughnessy contended that there was no ground for supposing that patients in cholera died as if poisoned with muriatic acid, or that there was evidence of any excess of that acid in the contents of their stomachs. And as to the absence of an eruption, he would instance hooping-cough, a decidedly contagious complaint, which was also unattended with an eruption. On the subject of cholera, Dr. O'S. expressed himself to be an advocate for its contagiousness: he instanced its making its appearance in the track of human intercourse at the greatest heights and lowest depths; and the recent information which had reached us with regard to the propagation of the disease among the pilgrims at Mecca, and thence spreading into Egypt, seemed to be decisive.

Dr. Granville, in explanation, maintained that pertussis was not contagious; and attributed the pestilential cholera among the pilgrims to their numbers, last season, and their long-continued prostrations over broiling sands, at 31° of Reaumur.

Dr. Gregory thought that the contagious cholera was a *new disease*, sent by Providence to visit the earth, as the small-pox was in the seventh century. The cholera, too, would probably abide with us, like the small-pox, appearing from time to time, after its first ravages, in a virulent form. The cholera had originated in India in 1817, and it still continued to exist in that country, and most likely would never be eradicated. The same, no doubt, would be its destiny in Europe. With regard to quarantine, he feared no measures would be sufficiently rigorous to exclude the disease: nothing short of an impenetrable wall built all round the country could effect such an object.

Dr. Johnson called the attention of the Society to the difficulty of coming at exact information with regard to the mode in which the disease was propagated: the evidences to be found in books were suited to the writers' preconceived opinions. He took the latest book published on cholera—that by Mr. Keunedy—and what did he there find? The author professes to give a selection from the excellent Indian Reports, and, out of some hundreds, thirty-seven are given. Now it is a well-known fact, that the great majority of Indian practitioners are non-contagionists; yet, what has Mr. Kennedy done?—of his select thirty-seven, all but two are contagionists. As to cholera being a *nova pestis*, as Dr. Gregory would have us believe, he (Dr. J.) could not subscribe to any such doctrine. He had *seen* the disease himself, in India, twenty-seven years ago, and Mr. Curtis had had experience of it twenty years earlier. Nay, he was not sure but that Sydenham had witnessed it, in the peculiar cholera of 1669, as he says nothing about the presence of bile in the discharges of the stomach, in his account of it. Precautions for the exclusion of cholera are useless: even if the impenetrable wall of Dr. Gregory were built as high as the sky, the disease will come among us *if it chooses*.

Dr. Stewart made some remarks on the probable influence of certain vicissitudes which had taken place in the weather within the last year or two. Severe cold had been experienced in southern climates, and extreme mildness in the northern latitudes.

Dr. Johnson (after a pause in the debate) rose to express his opinion that the propagation of cholera can only be accounted for by the supposition of exhalations from the earth: there is something going on beneath the earth's surface that we know nothing about.

in either case as to leave no doubt upon the minds of many that cholera both is and is not an infectious disease. Nothing renders this more probable to my mind than the mutual retraction of opinion that has prevailed among several medical men of observation and experience who, starting with the conviction of its contagious or non-contagious character, have subsequently seen reason for adopting a conclusion the very opposite of that with which they had set out. Now if we admit that both parties are correct in the statement of facts, how can such apparent differences in opinion be adjusted? or admitting all the statements indifferently as facts, to what circumstances is it attributable that cholera spreads in some instances with such fearful velocity from person to person, while at others it appears scarcely to take the character of a contagious disease?

The only way I can see of reconciling the dissension which at present prevails respecting the mode of propagation of this disease, is to suppose, that when cholera first makes its appearance, while it occurs as yet but in a few sporadic cases, and in different parts of a town or district, it is highly contagious and speedily diffusive, because the full effects of its noxious influence fall suddenly upon untried constitutions; but when the distemper is become epidemic, when the fomites of contagion have become very extensively multiplied, and when the very atmosphere has become charged with seeds of the disease, then it is that those who have hitherto escaped the contagion are on that very account exempted in a measure from the risk which had otherwise attended a nearer approximation to the infected; as the beginning with small doses of any poison will enable a person eventually to receive into his system even a considerable quantity with impunity. Hence the immunity of many in infected towns, whose susceptibility to the disease gradually decreases, while the air around them is continually deteriorating, till at last they can approach with impunity to the bed-side of the infected with comparatively little fear or danger. But this immunity, however general it may become from the above and other causes, does not prove the disease to be non-contagious; and, indeed, if healthy individuals with untried constitutions, and who had been living in a purer air, were suddenly brought into close proximity with the disease, they would in all probability imbibe the contagion, and disseminate it to others similarly circumstanced to what they had been previous to exposure.

Note.—It is usually stated that the miasm of contagion is confined to a very limited sphere of action, varying in different diseases, but rarely extending many feet beyond the individual infected. I have, however, ventured to assume that such miasm may be greatly diffused, so as even to affect the whole atmosphere of a town, and in proportion to the malignity with which the epidemic may be raging. This appears to me probable—1, from one very presumable quality of such infecting agent which, as it must be extremely subtile, must be proportionably liable to almost indefinite dispersion, for even solid matter is almost indefinitely diffusible if it be diffusible at all in fluids: hence the operation of the finest chemical tests. 2. From the ascertained quality of a capacity in such infectious agents, that what can be concentrated till it acquires the fatal activity almost of prussic acid, may also be diluted down to harmlessness. Now the sudden deaths that have occurred during pestilences to persons in the act of unpacking merchandise, are generally admitted to arise from the over-powering activity of the concentrated form in which the miasm has made its attack. 3. Lastly, why but for this reason are persons who live in an infected town subject, as it has been remarked, to unwonted general indisposition of an indefinite character, and often so considerable and of such a kind as to need but a slight aggravation of symptoms to bring on the very epidemic that is prevailing? I conceive this to arise, because the air is to a certain extent corrupted by the diffusion of the epidemic poison, though not in all cases sufficiently so to induce the epidemic itself, but, in place of it, engendering other complaints, whose type and severity is proportioned to the susceptibility of the individual, and the dilution, so to speak, of the poison.

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CHOLERA.

To the Editor of the London Medical Gazette.

Brighton, Nov. 22, 1831.

Sir,

In consequence of Dr. Copland's allusion to my remarks on cholera, which you were pleased to insert in number 206 of your Gazette, I think it necessary to state that I did not see his article in the Foreign Quarterly Review, but that I am aware part of the treatment I proposed had been previously recommended.

The following queries having offered themselves to me, I beg leave to subjoin them:—

1. The depressing effects of the south-east wind (the scirocco of Italy) which have, probably enrrectly, been attributed to some electrical peculiarity, being more or less experienced in every climate, is there any connexion to be traced between this circumstance and the fact that pestilential cholera, comparing the point from whence it started with its present station, has upon the whole pursued a north-west track, accounting for the numerous deviations from it by lateral propagation by means of the various vehicles of infection?

2. Are there any data which tend either to overturn or to support the hypothesis that this epidemic is caused by a change in the electrical constitution of the atmosphere; that the expression used by some, comparing the state of the patient to that of an "exhausted Leyden jar," possibly may convey an important truth, and that electricity or galvanism, judiciously employed, may prove a most useful remedy? Galvanism would, moreover, seem suited, from the relation which exists between it and the nervous, especially the secreting power, and from the restoration of the secretions, particularly that of the kidneys, being of the first importance.

To revert once more to the question of contagion. Though we must concede to the non-contagionists, that this epidemic, as well as lues, plague, &c. not being coeval with man, the first case of those diseases which ever occurred *must* have been sporadic, and have arisen from other causes than contagion; and if this happened once, it may again at any time: still I do not see how any one can deny that this and many other diseases may, under certain circumstances, become infectious, or doubt, at this time of day, that cholera has long since assumed that character, and at present is chiefly propagated by contagion. Since, however, ocular proof of actual transmission from one individual to another cannot be procured and conviction cannot be forced on any one, the discussion of this subject will now, I hope, give way to topics more immediately connected with the treatment of the disease.

By inserting the above, and noticing the errata* at foot, you will oblige me.

I remain, sir,
Your most obedient servant,
E. Stanley, M.D.

ORIGIN OF THE VARIOLÆ VACCINÆ.

To the Editor of the London Medical Gazette.

Sir,

I trust to your sense of justice to give insertion to the following statement. All who have read your number for November the 5th, will immediately perceive the object I have in view. As my design is to be brief, I will deliver what I at present have to say in as few words as possible.

The history of the origin of the variolæ vaccinæ, divested of all ambiguity and confusion, stands thus. Dr. Jenner found that a disorder caught from horses and cows in the dairies of Gloucestershire, prevented persons so affected from being liable to small-pox. After long and patient, and successful investigation, he came to the conclusion that the disease so caught was a mild variety of the varinlæ which attacked men. This belief afforded him the means of unraveling all the difficulties connected with the practice of the cow-pox inoculation, and ultimately to present to the world the most comprehensive and perfect specimen of medical investigation that has ever been witnessed. I say this without fear of contradiction, inasmuch as scarcely one fact connected

* In No. 206, p. 176, line 21, *for* "embrocations," *read* "liniments;" line 28, *for* "counter-indicated," *read* "contra-indicated;" line 30, *for* "venæ," *read* "vena;" line 40, *for* "oxide," *read* "oxyde."

with the nature or modifications of the disease has been recorded of which he was not aware.

His opinions concerning the origin of the disease necessarily rested upon a different foundation, and could not, therefore, be so conclusive as those which were based on fact and observation. He conceived that the variolæ, in all their forms, were ultimately to be traced to the horse. I need not remind your readers how much he was ridiculed for this and many of his other doctrines, but there was a foundation for them that they little dreamt of.

Matters continued in this state of uncertainty up to the period of his death, with this additional difficulty, that there was an increasing unwillingness in the minds of medical men to subscribe to many of Dr. Jenner's most important positions. Since that period, the great problem touching the origin of the variolæ vaccinæ has been solved, and the soundness of Dr. Jenner's views has received the most signal corroboration.

I am not aware that the facts that I am about to state, were ever before the period alluded to brought to bear *on the origin of cow-pox, and on the identity of that disease with small-pox*; but be that as it may, it has been incontestibly shewn.

First, that cows in many parts of Europe have long been known to be liable to *small-pox*.

Secondly, that this disease has spread from flock to flock in the most fatal and *pestilential* manner.

Thirdly, that it raged in England not much more than fifty years ago, and that the *practice of inoculation was had recourse to, to subdue its violence*.

Fourthly, that it is even now occasionally a violent and fatal disease among the cows, and for the most part is propagated by contagion through the whole herd, when it breaks out in a dairy.

With the exception of the last fact, I believe every one of the above-named particulars were *hid* from the profession till within these four years. At least I do not know that the slightest reference has been made to them by any writer anterior to that time, *in order to demonstrate the nature and origin of the variolæ vaccinæ*. Their application to this subject is clear and conclusive. Dr. Jenner finds the remains of a more violent epizootic small-pox in his neighbourhood. Without being aware of the actual history of this malady in the inferior animals, he investigates its nature with matchless success, communicates it by inoculation to man, and affirms that the disease thus communicated will afford protection against small-pox as complete as that disease itself does.

What was the ground of this confidence?—*His conviction of the identity of small-pox and cow-pox*. On what was that conviction founded?—*Analogy, and the ascertained peculiarities of the disease*. How has this conviction been strengthened and demonstrated?—*By tracing the small-pox in cows for hundreds of years*, and affording the strongest presumptive evidence that the disease was sometimes communicated *to man, in its most fatal form, as it has likewise been done in the mildest form!*

It is very painful for me to be compelled thus to address you, and still more that I should find it necessary at this late period to refer to a work of my own for proofs of the accuracy of these statements. The interesting fact recorded by Dr. Sonderland is demonstrative of the truth of the conclusions which have been mentioned, though some of his inferences are at variance with the best historical evidence.

I know that when an author complains he is generally supposed to be impelled by over-weening conceit or mortified self-love. I am not, I trust, one of that class. I value truth and justice above all things; and provided they are duly upheld, all personal feelings would instantly give way. On this occasion I am satisfied that I am not seeking too much from you, or from your contemporary who first published the document to which this letter refers, when I beg that you will honestly and faithfully compare it with my published work; and having done so, I would ask you, or any competent judge, whether it was right and becoming to withhold all the evidence which that work contains?—I am, sir,

Your obedient humble servant,

JOHN BARON.

Gloucester, Nov. 21, 1831.

SMALL-POX.

To the Editor of the London Medical Gazette.

Sir,

The spirit of your Journal seems so fair, and your desire to maintain the truth is so manifest, that I feel assured you will permit me to make a few remarks upon a paper that appeared in your last number, although it has been avowedly taken from the publication of a contemporary.

I believe it to be a mistake to affirm that the variolæ vaccinæ have originated in the manner referred to in the seventh aphorism of Doctor Sonderland. It is at least certain that one of the earliest and most authentic historical documents, which by the best judges has been pronounced to be descriptive of the small-pox in man, proves that the same disease simultaneously appeared in the inferior animals. But, be this as it may, traces of eruptive epizöotic diseases, in many cases communicated to man, may be found in the historians of almost every age. As we deseend nearer to our own times, the descriptions are more precise and accurate; and the variolæ among cattle is a disease as clearly described, and as accurately defined as any variolæ that ever attacked man. And it is a most remarkable fact, that in the year 1690 it began with the animals, and immediately afterwards proceeded to attack the human species in the most fatal form. It is little more than fifty years since this epizöotic pestilence ravaged the cows of a large portion of Europe, and especially this country; and it has been proved, as far as any thing of that kind can be proved, that the cow-pox which Dr. Jenner met with in our dairies was the local, or rather *enzöotic* remains of that pestilence.

I believe, sir, it is equally a mistake that the variolæ which appears among cows is always of the mild character that it has lately assumed, as is implied in the sixth aphorism of Doctor Sonderland.

During the last epizöotic among the cows in this country it was of a most virulent and contagious character; and the practice of inoculation was frequently and successfully employed, as in the human subject, to subdue its severity. It is equally true, that the disease has, within these few years, been so violent in the dairies of Gloucestershire as to destroy the life of the animal, a case of which kind has been published in the Life of Doctor Jenner, page 351. The disease, likewise, when caught by the milkers, is often a severe one; and at times there is a considerable eruption. Were I to detail all the facts by which these statements may be proved, I should more than fill your number. They, and a great many others, have been examined, and arranged for the express purpose of proving the identity of small-pox and cow-pox, and thereby explaining the protecting virtues of the latter. The whole will be found in the fifth, and other chapters of the volume above cited. I have examined the evidence therein contained with great attention; and it appears to me as conclusive as any thing of the kind can be. The propagation of the small-pox to the cow, in the manner deseribed by Doctor Sonderland, is an interesting and important fact, and affords the strongest confirmation of the accuracy of the doctrines coutained in Dr. Baron's work.

I am, Sir,
Your obedient servant,
Non Nemo.

November 10, 1831.

PHYSIOLOGICAL CONJECTURES.

No. II.

By Philalethes.

To the Editor of the London Medical Gazette.

Sir,

I.

It has long been an interesting question to determine, what is the precise state of the circulation and of the respiration, during the state of torpor in hybernating animals.

I think this may be done, in regard to the circulation, by submitting the web of the frog, the wing of the bat, the ear of the dormouse, to be viewed in the mieroscope.

I think the quantity of respiration may be readily and accurately determined, in the manner described in my former communication, in the Medical Gazette, No. 185, page 373.

compound powder of Ipecacuanha, in nauseating doses, has proved beneficial, not only in restoring the balance of the circulation, but in aiding expectorants in unloading the bronchial tubes of their overbundant mucus. With regard to the kind of expectorants to be employed, you have seen these varied in almost every case: where any thing like fever bas existed, Ipecacuanha has been selected; and where tbe babit was languid and the pulmonary exhalants required to be stimulated, every thing has been derived from combinations of Squills and Ammoniacum. Dr. Cullen has extolled the fœtid Gum Resins in such cases; but I have scarcely ever found it necessary to order them, the Squill and Ammoniacum having answered every intention; or, where more stimulus has been demanded, I have generally relied on the Carbonate of Ammonia. Where the languor was not so great, and a tonic rather tban a stimulant was indicated, whilst at the same time the expectoration required to be attended to, a combination of Nitric Acid, Tincture of Squills, and Henbane, has answered well; and where expectorants bave not been indicated, an equally good effect has resulted from Sulphuric Acid, with Senega, or some other bitter. Tonics in tbe intervals of the paroxysms, in conjunction with light diet and regular exercise, are the best means of allaying tbe morbid mobility of the system; and if any thing can effect such a change, they present the best means of overcoming the predisposition to the disease. Too little attention is usually paid in regulating the exercise of Asthmatic patients; this should be strictly forbidden after a full meal, as it invariably produces an anxious, agitated, and difficult respiration; and therefore, in the class of patients that present themselves to us, and who must return to labour immediately after dining, tbe importance of eating sparingly sbould be strenuously urged. "I must eat to enable me to work," is the reply in these cases; but you must point out to the patient, tbat the same disadvantage arises from overloading the stomach as from starvation; in the one case no strength follows, because the oppression destroys the powers of digestion; in tbe otber there is notbing to digest; but in both the effect is tbe same. With respect to exercise at other times, tbe Asthmatic who is wholly regulated by his feelings is too apt to shrink from it, on account of the uneasiness whicb it occasions; but this feeling he must be taught to disregard; and by encountering it with firmness, be will gradually familiarize himself witb the feeling, till he at length gains tbe ascendancy over it, and profits by the effort. In the middling and bigher ranks of life, notbing tends more effectually to lessen the tendency to the returns of the Asthmatic paroxysms, than the use of the shower-bath.

Porrigo Larvalis.

The great number of cases of cutaneous eruptions which present themselves at the Dispensary, furnisbes you, gentlemen, with excellent opportunities of studying this class of diseases. It is my intention, occasionally, to bring them particularly before you. The case of Porrigo which you have just seen, and wbich has so rapidly yielded to the remedies prescribed, is a striking demonstration of the facility of managing that formidable-looking eruption. The patient, a boy of fourteen years of age, presented himself at the Institution only six days ago, and is now fit to be discharged as cured; but I retain him to prevent too immediate a return to the diet and habits to wbich I ascribe the disease. Owing to the poverty of his parents, he has been scantily nourisbed; his food also has been of the most indigestible kind; he has, besides, been daily purged with salts, on the supposition of bumour being present in tbe blood; so tbat tbe poor boy was in a very reduced state. Wheu he presented himself at the Dispensary, tbe thick, honeycomb crusts, which follow tbe psydracious pustules, surrouuded the mouth, covering nearly tbe whole of the lower part of the face; and the pain of opening the mouth was so great, as to prevent him from putting out his tongue. A few crusts were also scattered over the forebead and tbe scalp, and two or three blotcbes on each arm. Tbe countenance was pale and sallow; the expression anxious. He complained of gnawing pain at the stomach; and was much distressed with sour eructations.

As tbis eruption is altogether dependent on tbe state of the stomacb, tbe indications of practice are cbiefly directed to correct the deranged condition of that viscus, and gently to stimulate tbe diseased surface. With tbis view, ten grain doses of tbe Hydrargyrum cum Creta, witb a quarter of a grain of Opium, were directed to be taken at bed-time every night: and a powder, consisting of a scruple of Carbonate of Soda, five grains of Rhubarb, and a scruple of powdered Columba root, twice a-day, with a generous, but light animal diet. The crusts were ordered to be smeared with the diluted ointment of Nitrated Mercury, night and morning. In tbree days, tbe crusts fell off, leaving a red, rough surface, which is now assuming a more bealtby aspect, and the dyspeptic symptoms are rapidly disappearing. Tbe object of the alkali, in this case, is not to neutralize tbe acid, but to allay tbe irritability of the stomacb, and tbence to enable it to prepare a more healtby gastric juice. The result of this practice, both in tbe case under consideration, and in similar cases, wbich you have witnessed, is well calculated to inspire you with confidence in this mode of treating this disgusting eruption.

Epilepsy.

The man who had been discharged as

cured of this disease, has again presented himself to the Institution, and states that he had three severe fits two days ago: his face is cut from the fall caused by the fit, which came on in the street. On making inquiry into the cause of this recurrence of the disease, I am persuaded that it is to be attributed solely to intemperance, notwithstanding the positive assertions of the man to the contrary. Whilst he was affirming that he had not tasted spirits, for many weeks, his breath smelt like a gin-bottle; and consequently every confidence in his account of himself was at an end. Nothing is more difficult than to get at truth, in such cases; and you must exercise a sound discretion in ascertaining the nature of exciting causes amongst this class of the community. Since this discovery, I have little expectation of being able to cure the disease in this individual.

IDENTITY OF SMALL-POX AND COW-POX.

To the Editor of the London Medical Gazette.

SIR,

THE excitement produced by the irruption of cholera having in some degree subsided, you will perhaps permit me to refer to a paper which appeared in your journal of the 5th November, purporting to be an account of the experiments of Dr. Sonderland, of Bremen, to determine the identity of small-pox and cow-pox. With this I would beg to couple a letter from Dr. Baron, of Gloucester, published in the Medical Gazette of December 3, in which the same question is discussed. From the perusal of these communications, a superficial reader could hardly rise without the conviction, that the identity of small-pox and cow-pox was hereafter to be viewed as an undoubted truth—a perfect *axiom* in pathology. My object in this letter will be to sift the facts and arguments of these gentlemen somewhat in detail, and to shew that much still requires to be done before this question can be laid at rest.

Dr. Sonderland's observations are given "*almost without abridgment*;" yet we are left in the dark as to *where* and *when* those experiments were conducted, which are said to have thrown so much light on the theory of small-pox and cow-pox. We are left to guess whether the experiment of infecting cattle with variola has been performed once, twice, or oftener—whether the disease thus produced is fatal, or otherwise, to the animal—whether, when thus artificially produced, it spreads by contagion. Our imagination is taxed to discover whether the vaccination of children from this singular source has been performed once only, or so often as to preclude all reasonable doubt—whether the vesicles thus produced present any, and if any, what modifications of their normal character; and lastly, whether the lymph thus generated, has, or has not, been kept up by successive inoculations. It is surely, sir, not too much to expect, in a matter of so much importance, that these questions should be satisfactorily answered before we are called upon to *give in our adhesion*. The whole affair seems to me to savour very strongly of *romance*. We are told, for instance, "that a bed-cover impregnated with variolous contagion, if firmly rolled up, then wrapped in linen, afterwards in paper, then duly packed in a bucket, and carefully put by in a cool, shady place, with the thermometer between 32 and 52 degrees, will retain the contagion in an active state for at least two years." Now, sir, I would venture to ask, how many bed-covers must have been rolled, wrapped, packed, and stowed away, before this dogma in epizootic pathology could have been concocted? Dr. Sonderland, in this paragraph, seems to me to prove a great deal too much, and by so doing throws discredit upon the whole of his statements. The deductions of Dr. Sonderland from these alleged facts are miserably lame and impotent; in truth, it is only the first and third of them which in any way invite our attention. To these Dr. Baron has given a degree of importance, by putting in a *counter-claim* for Dr. Jenner. How far it is prudent to mix up the revered name of Jenner with a theoretical question, involved in so much doubt as the identity of small-pox and cow-pox, it is not for me to decide; but as Dr. Baron has put forward certain statements in reference to this question, it is proper to examine them, and, as far as possible, estimate their true value.

Dr. Baron states confidently (Medical Gazette, vol. ix. p. 301), "*that cows in many parts of Europe have long been known to be liable to small-pox.*" That cows are liable to a disorder which by some persons is *believed* to be identical with small-pox, is most true; but that this disease has been ever *proved* by *unquestionable* experiments to be genuine variola, I must continue to doubt. Analogy, the ascertained peculiarities of the

ford, indeed, presumptive evidence; but such evidence may be satisfactory to some persons, and very unsatisfactory to others. Nothing short of the *direct* transmission of the disease from man to animals, and from animals back to man, can (in my mind) justify the position, that *cows are liable to small-pox;* and where, may I ask, are such experiments recorded? Dr. Sonderland states (and here I believe him to be quite correct), that, "although many had suspected the identity of small-pox and cow-pox, no one had previously decided the question experimentally beyond doubt." The *inoculation* of cows has never been satisfactorily proved. Whatever may have happened to this or that observer, it is perfectly well known, that no person can at pleasure communicate the cow-pox to a cow by inoculation. I am free to admit, that Dr. Sonderland's experiments (if correct) have settled the question; but I cannot consent to build a superstructure of such vast extent and importance, upon so insecure a foundation as the experiments of a single individual, miserably deficient in details, and overloaded with theoretical deductions.

The second, third, and fourth positions of Dr. Baron (Medical Gazette, *loc. cit.*), in their bearing upon the question at issue, are contingent upon the first. Unless the disease of cattle therein alluded to be genuine variola, it matters not (so far as this question is concerned) whether it be, as Dr. Baron contends it is, a contagious, innoculable, and still prevalent disorder. A correspondent, under the signature of Non Nemo (whose style of writing savours very strongly of my old acquaintance, M. D. Oxon), corroborates all that Dr. Baron has affirmed; and adds, that the evidence in favour of the identity of small-pox and cow-pox, to be found in the fifth chapter of Dr. Baron's Life of Jenner, appears to him to be as conclusive as any thing of the kind can be. I regret that I cannot join in this opinion, though I have studied that chapter most diligently. For Dr. Baron's talents and professional attainments, and zeal in one of the best of causes, I entertain the highest respect; but he appears to me to attach an importance to the determination of this question which is scarcely warranted. The *practice* of vaccination would neither be promoted by the general adoption of Dr. Baron's views, nor retarded by disease, and epizootic history (the three points upon which Dr. Baron relies), af- their abandonment. The hypothesis is ingenious, but the importance of *demonstrating* it may be estimated from this single consideration—that vaccination has been practised for thirty-two years, and has established itself in every region of the earth—*et adhuc sub judice lis est.*

I have the honour to be, sir,
Your very obedient servant,
GEORGE GREGORY.

31, Weymouth-Street,
Dec. 22, 1831.

CASES OF STRICTURE PERFORATED BY THE LANCETTED STILETTE.

To the Editor of the London Medical Gazette.

SIR,

IF you consider a few of such cases as the two following, where permanent strictures of the urethra have been perforated by the lancetted stilette, worthy of your notice, I shall occasionally send you some for publication, and you will oblige me by their insertion in your valuable journal.—I am, sir,

Your obedient servant,
R. A. STAFFORD.

28, Old Burlington Street,
Dec. 21st, 1831.

CASE 1st, August 1830.—L. Snowdon, æt. 40, a printer*, applied to me with a diseased urethra, which began to be contracted at one inch from the meatus urinarius, and extended nearly two inches along the canal. Externally the part was extremely indurated; and when pressed between the finger and thumb, it felt as if a piece of small cord had been lodged in that portion of the urethra. From the patient's account, the disease had originated in consequence of a very virulent gonorrhœa, which he had contracted three years ago, and from which time the passage at that part had gradually narrowed, until it became so closed as only to allow of the urine passing through it guttatim. In the early stage of the disease bougies had been passed; but for the last two years no instrument whatever could be got through the stricture. The poor fellow was much emaciated, and appeared to have suffered considerably.

As my friend, Dr. Eckström, professor

* One of the compositors on the Medical Gazette.

of surgery at Stockholm, was very desirous to see the treatment, I requested him to examine the case. He was of opinion, from the great extent of disease, its situation in the urethra, its indurated structure, and its impermeability, that no other treatment could, with success, be adopted than its perforation by the lancetted stilette. I accordingly at once cut through in his presence about half of the strictured portion, leaving a bougie in the divided part for one night, to keep it open. On the following day it was withdrawn, and introduced daily. In a week from the first operation, I completed the division of the part, and introduced a No. 9 catheter into the bladder. Steel dilators were now passed on the alternate days until they arrived at the size of No. 11. The patient, finding himself nearly well, discoutinued his attendance on me. I have since heard he is quite recovered, and that he makes his water in a natural sized stream. The treatment occupied from three weeks to a month, and during the whole time no unfavourable symptom whatever occurred.

Remarks.—From the preceding history of this case, it may be observed that two inches of the urethra was irregularly thickened, and contracted to such an extent that the urine could only infiltrate itself through it guttatim. The part, of course, could not be dilated, for it was impermeable to a bougie. The extent of the disease also prevented the application of caustic, for it would have been almost impossible to have burnt through so long a contraction with this substance. The division of the part externally, likewise, (if ever to be recommended,) would in this instance, from the situation of the disease, have been of no service, and most probably attended by serious consequences. What treatment, then, was left for the poor sufferer? None of the above could have given him any relief. In course of time the urethra would have entirely closed up at that part, and then ulceration behind the stricture would have taken place, giving rise to extravasation of urine, with fistulous passages; or he would have lingered on, as many others have done before him, and at length would have been worn out by the disease, and have died. It will be seen the simple way by which the employment of the lancetted stilette permeated an obstruction which could not by any other means have been overcome.

Case II.—A naval officer, æt. 60, consulted me in Aug. 1830, under the following circumstances. He had been the subfor many years, of an impermeable stricture, situated immediately behind the bulb, at the entrance of the membranous portion of the urethra. He had placed himself under the care of various surgeons of eminence, who had tried all the known means, excepting external division, without success. As a last resource, and as otherwise he must have refused a valuable naval appointment, he was desirous of having the stricture divided by the lancetted stilette. The case was by no means a favourable one for the operation; for after each introduction of the bougie, hæmorrhage, to the amount of from half a pint to a pint, always occurred. Having had, however, another case very similar to it, where the bleeding ceased immediately after the incision, I consented to operate, more particularly as his nephew, who was a surgeon, was to be present. On the first day about half an inch of the obstruction was divided, and the hæmorrhage certainly was less than when a simple bougie was passed. On the sixth day from the first incision I introduced the instrument, and made another, when it slipped on with the greatest ease into the bladder; but little blood followed. A No. 9 catheter was now passed on the alternate days. Six days after the operation he was attacked with a continued fever, which he had been liable to before, in consequence of living on the banks of the Thames. He was removed from that situation, but the fever ran on for a month. During this period I did not attempt to pass instruments, but the stream of urine was all the time nearly as large as natural. When he was convalescent I introduced a bougie, but I could not, in consequence of spasm, get it into the bladder. He was recommended, therefore, to go into the country to recover his health, and he paid a visit to his brother-in-law, who was a surgeon. After a little time he improved so greatly, that this gentleman twice passed a No. 9 catheter. Having accepted the naval appointment, and his time being expired, he sailed for Jamaica. Since his arrival I have heard

the writer's observations as accurately as possible, to lay before our readers.

* * * * *

"Our author observes, that one common property of *neutral salts* is that of giving a rich arterial colour to venous blood. This property is common to them all; and the degree to which they possess it is, perhaps, the best test of their purity as saline agents. To ascertain the effects of different agents on the blood, he made a number of experiments, in which it was observed—

"1st. That all the acids give a dark colour to healthy blood, and, in proportion to their strength, change it from red to black, as certainly as they change vegetable colours from blue to red. Even the vegetable acids so completely blackened the blood, that the addition of a little water converted the whole into fluid exactly resembling the black vomit. Secondly, *the pure alkalies have a similar effect with the acids, in changing the blood from red to black, though not in the same degree.* Thirdly, the neutral salts immediately changed the venous blood from a dark modena red, to a bright arterial colour. Even those salts that contain a slight excess of alkali, the sub-carbonate of soda for example, immediately give to venous blood a beautiful bright arterial colour. The effects of these experiments are best seen when made on healthy blood. The agents ought first to be dissolved in a little soft water, and then well mixed with the warm blood, before it begins to coagulate.

"4thly. When the neutral salts are mixed with the dark and dissolved blood that had been taken from the hearts of those who had died of yellow fever, even the black and dissolved fluid was instantly converted from a black to a bright arterial colour.

"The nature of this paper (said Dr. S.) prevents me from entering minutely on the important effects which this saline impregnation produces in the vital fluid; but, in a work which will soon be published, I shall endeavour to prove, first, *that the blood owes its red colour to this saline impregnation.* Black appears to be the natural colour of the colouring matter; for, when we take a clot of blood, and deprive it completely of its saline matter, by immersing it in fresh water, the colouring matter soon becomes so black, that even oxygen has no effect in changing its colour. But, when we immerse this black clot in an artificial serum, made by dissolving some saline matter in water, the black clot in this clear fluid assumes almost immediately a beautiful bright arterial colour. Secondly, that, to this saline impregnation the fibrin owes its fluidity: for it retains this form only so long as it is held in solution in the salt serum. Thirdly, that the change of form which this saline matter undergoes, when the blood changes from arterial to venous, and from venous to arterial, alters its capacity for caloric, and gives it an influence in supporting the temperature of the system. *The saline impregnation also adds to the stimulating quality of the blood,* and assists, even in a high temperature, in adding to its powers of self-preservation.

* * * * *

"As we have no doubt that Dr. Stevens will pursue this interesting inquiry further, and lay the results of his observations before the profession in a more extended form, we shall abstain from any comments on the present occasion. We have laid a very full and faithful account of the paper before our readers, and leave them, for the present, to draw their own conclusions."

As I trust that many of your readers will feel an interest on this subject, which I have endeavoured to divest of every thing like unbecoming personalities, and as most of them have not seen the West India fevers, it may now be proper for me to make a few observations on the nature and treatment of these diseases, but above all to point out the *fatal error* in the old practice which has been, as I believe, the true cause of one-half, and perhaps even more, of the mortality in the West India fevers. This I shall do as early as possible, and when, I hope, your readers will find in my remarks something to interest them, more than the controversial matters on which I have, in vindication of my character, been most unwillingly compelled to enter.

I am, sir,

Your obedient servant,

W. Stevens, M.D.

Albany-Street, Jan. 9, 1832.

ADDITIONAL REMARKS
UPON
VACCINATION.

By William Howison, M.D. F.R.C.S.
Vaccinator to the Royal Dispensary of Edinburgh.

To the Editor of the London Medical Gazette.

January 1st, 1831,
9, Nicolson-Square.

Sir,

This winter, since commencing my duty on the 1st of November, as Vaccinator to the Royal Dispensary of Edinburgh, I have again turned my attention to the appearance of the vaccine vesicles upon the arms of the numerous children vaccinated at that institution, throughout all their stages. During the months of November and December, which have now passed away, I feel happy to state that the various vesicles throughout all their stages, have presented a much more favourable appearance than they did last winter during the above-mentioned months, as described by me, in the paper on Vaccination, which appeared in the Medical Gazette of July 1831, to which I request my readers again to refer, as the precursor of the remarks which I am now about to make. The vesicles of the past months of November and December have invariably appeared large, beautiful, and distinct, with pellucid virus, as much so as they did during the pleasant genial weather of June and July. And vaccinations, at a distance performed with virus taken from them, have proved equally beautiful and satisfactory. I have no hesitation in attributing this fortunate circumstance principally to the mild temperate weather, the wind being confined to the south, south-west, or west, which, with the exception of a few days of intense frost and snow following a northerly wind, about the beginning of November, has prevailed throughout November and December, resembling more the temperature of spring than that of winter. Small pox has besides ceased to exist for several months past in the city of Edinburgh and its neighbourhood, and perhaps the removal of that destructive disease from the environs of the Dispensary, may have rendered the cow-pox vesicles more complete and satisfactory than when it was extending its violence round about, as it did last winter, when my remarks were made.

A few medical friends, interested in this subject, having done me the honour of reading the ideas that I have already thrown out upon vaccination, having requested that I should pay attention to the shape and size of the vesicle, the state of the cicatrice; how far vaccination is modified or destroyed by other diseases existing in the infant at the same time; the nature of what is termed modified small-pox, and the identity of small-pox and cow-pox; I now proceed to make remarks upon these, so far as my limited means and abilities will allow.

I.—*The Figure and Size of the Vaccine Vesicle.*

The figure and size of the future vaccine vesicle depends entirely upon the mode of inserting the virus at the time of performing vaccination. The vaccine vesicle is by no means confined to any particular figure or dimensions: it is not uniformly circular, as many authors have erroneously supposed and described in their writings; neither is it oval. I will say that it will assume any figure which the puncture or line of incision at which the virus is inserted may be made to take; forming an elevated boundary to that line. Neither is its size of any limited extent: were a circular line drawn round the arm, or even round the wrist, of an infant, and vaccine virus inserted throughout the track of that line, we would have the future vesicle following a similar extensive circular range. In public Dispensaries, a vesicle of an oblong shape, formed by inserting the virus by the incision of the round-pointed inoculating lancet, is absolutely necessary, to afford a sufficient quantity of virus for carrying on future vaccinations. In private practice, this circular vesicle, formed by the puncture of the common bleeding lancet, perhaps may be sufficient. The bleeding lancet, however, I neither use nor approve of, for reasons already given to the public.

II.—*State of the Vaccine Cicatrice at an after period.*

With regard to the vaccine cicatrice at an after period, during the remaining life of the individual, proving that the vaccination has been properly per-

formed and passed through, my experience is limited. I have no hesitation in stating, that the regular punctured cicatrice, formed by the destruction of the cutis vira, which can at no after period be regencrated, and subsequent contraction of the cuticle, which gradually takes place during the formation and progress of the vaccine crust, previous to its being thrown off, from its white colour, &c. continning unchanged for the life of the individual, is a certain and infallible criterion to the accustomed observer that the vaccination has been properly gone through, and that no other disease or accident, so far as we know in the present enlightened state of medical knowledge, will ever assume the same regular appearance. To this circumstance, however, I must confess I have not as yet given my complete attention.

III.—*How far the Progress of Vaccination is modified, or destroyed, by other Diseases existing in the Infant at the same time.*

Upon this subject I have little to say. It is a circumstance admitting of no doubt, that the state of perfect health is the one most congenial to the effieacy, beauty, and future progress of the vaccine vesicle; and in proportion as the constitution of the individual vaccinated deviates from that state, in the same proportion does the vaccine vesiele deviate from its beauty and efficacy. My friend Dr. Sanders, who many years ago favoured the world with his observations upon this subject, in a conversation which we lately had, informs me that he repeatedly, by way of experiment, vaccinated infants labouring under varicella, or chicken-pox, in all its stages; and that both cow-pox and varicella went on together, throughout their whole progress, without interruption. I look upon varicella as a mild disease, notwithstanding the far more extensive experience of my friend. Whether, however, vaccination and other more severe constitutional diseases, as measles, scarlatina, &c. will go on in the infant at the same time, and pass through their regular stages, or whether one will destroy the other, my experience does not enable me satisfactorily to decide. Were I compelled to give my opinion, I would say they would overpower each other, the stronger gaining the ascendency at last. I finally believe that every other constitutional disease will either totally destroy the progress of the vaccine vesicle, or render it so imperfect as not to afford, at a future period, security against small-pox infeetion. This, in reality, is a subject of little practical importance, as no medical man can be justified, under any circumstances, of vaccinating individuals when labouring under the influence of severe constitutional disease; with the exception of scrofula, mania, or pthisis pulmonalis.

IV.—*Identity of Cow-pox and Small-pox* *.

V.—*The Nature of the Disease termed Modified Small-Pox.*

With regard to the nature of the disease termed modified small-pox, I subjoin the following letter, lately sent to me by Dr. Sanders, in answer to a request to be favoured with his ideas upon that subject. I decline accompanying it with any remarks of my own.

Duke-Street, Dec 23, 1831.

Dear Howison,—With regard to vaccination as a preventive of small-pox, I maintain that the disease of the real small-pox never occurred either in its complete form, or under any modification, in a person who had undergone vaccination.

That it never occurred in its complete form, is proved as follows:—1st, During 25 years I have not seen even one instance of the small-pox, in a person who had been vaccinated. 2dly, That in the writings of the anti-vaccinists, as well as those who believe in the occasional failure of vaccination, there is not one instance of the small-pox. Respecting the voluminous works of the new Professor of Pathology on this subject, I agree with the authors of the *Dictionnaire des Sciences Médicales*, "*that all the cases produced by Dr. Thomson want the proper characters of the genuine small-pox (varicella).*"

That it never occurred under any modified form, is proved as follows:—1st, If a certain disease is modified by vaccination, then a form of disease must have been produced, which was unknown previous to the discovery of the vaccine virus, and which cannot appear

* This portion of the paper we omit, because it is merely a copy of Dr. Sonderland's remarks, already published in this journal.—E. G.

except in the vaccinated. 2dly, In the vaccinated, no disease resembling small-pox has appeared, that was not known and described long before Dr. Jenner, the discoverer of vaccination, was born; nor does any variolid affection now appear in the vaccinated, that does not equally appear in those who never were vaccinated.

What is the disease called modified small-pox? On comparing the modern cases with those described above a century ago, we find that the eruption called modified small-pox, is no other than the disease distinguished by the name varicella, or chicken pox, which is neither prevented nor altered by the influence either of small-pox or cow-pox.

I have not leisure to correct or copy, but such as it is you have it, from yours,

Ever truly,

JAMES SANDERS.

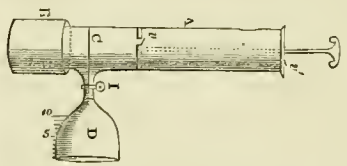
To Dr. Howison.

INSTRUMENT IN AID OF VENESECTION IN CHOLERA.

To the Editor of the London Medical Gazette.

SIR,

As the abstraction of a certain quantity of blood, in the early stages of cholera, is generally admitted to be a most desirable proceeding in the treatment of the disease; and as this quantity of blood, owing to the collapsed, or collapsing state of the patient, very frequently cannot be obtained by the ordinary method of venesection, I beg leave (with great diffidence, not being a medical man myself), through the medium of your excellent journal, to submit to the consideration of the faculty the subjoined plan of an instrument, by means of which, in conjunction with the ordinary method, the object in view may be attained.



A, represents a brass cylinder and piston, about seven inches long; C, a diaphragm, or partition, in the cylinder, continued into the lateral orifice to beyond the stop-cock, I; D, is a graduated glass receiver, the receptacle of the blood abstracted; B, a glass cylinder, about two or three inches in length, accurately fitted on to the brass barrel, A, and having its lower open extremity well ground. The lateral tube is to be about 7-8ths of an inch in diameter, divided into two compartments (5-8ths and 7-8ths), by the continuation of the diaphragm. Two valves opening upwards are marked by v and v'. D, the receiver, is applicable at pleasure to the lateral orifice, by means of an air-tight collar.

The principle upon which this instrument is intended to act being so well known, and the construction of it so simple, it would be superfluous in me to enter into a lengthy detail of particulars. It may, therefore, be merely necessary to remark, that the open lower extremity of the instrument being gently pressed around the orifice of the punctured blood-vessel, the piston is drawn back slowly, ejecting by this means a certain quantity of air. If the air withdrawn in this way be, to that still remaining in the receiver and glass cylinder (B), as one to two, of course the fifteen pounds pressure to the square inch, which the atmosphere opposed to the flow of blood from the vessel, is reduced to seven pounds and a half; and if it be desirable to procure a more abundant supply, or to promote the flow of blood still further, by restoring the piston to its former place, the operation may be readily repeated.

Before removing the receiver, for the purpose of emptying it, as it should not be allowed to hold too large a quantity at a time, care should be taken to turn the stop-cock, so as to close the passages.

And in conclusion, sir, allow me to suggest the general application of this instrument in all cases where the *rapid* abstraction of blood is desirable.

I am, sir,

Your obedient servant,

JAMES ALEX. VENTRESS.

London, Jan. 7, 1832.

As the only means of permanently overcoming the closure of the jaw, was the removal of the cicatrix, I determined upon excising and replacing it by sound integument from the face and neck.

Accordingly on the 8th of April, assisted by Dr. Vache, and in the presence of Drs. Hartwell, Barrow, Wilkes, Hosack, and several others, I performed the operation.

It was commenced by carrying an incision from a little within the upper angle of the mouth, around the outer margin of the cicatrix, to a little within the lower of the under lip, and by the immediate removal of the newly formed parts included within it. The adhesions between the jaws were next divided, which enabled me, in consequence of the relaxation thus produced, to insinuate between the teeth of the opposite side, the point of the lever used in my former cases, with which I finally succeeded in opening the mouth.

This point accomplished, the lips were brought together at the angle of the mouth by a suture, and I proceeded to detach a portion of integument sufficiently large and of corresponding shape, to replace the part removed. See dotted line, fig. 1. It was turned into the space it was intended to fill, leaving a tongue three-quarters of an inch in breadth, connected with the adjacent part and sufficient for all the purposes of circulation. The cut edges were adjusted with extreme accuracy, by means of interrupted sutures and adhesive straps: the lower wound was contracted as much as possible by adhesive plaisters, and the whole covered with lint, compress, and bandage.

Previous to the operation she took Sol. sulph. Morphiæ, double strength of Magendie's formula, gtt. xiv. The operation occupied about an hour, and was sustained with a firmness peculiar to the female sex.

Evening.—Had been sick at her stomach, and vomited some coagulated blood, which had no doubt been swallowed during the operation.

9th.—Hardly any perceptible swelling of the face. As far as the parts can be seen all looks favourable. She can depress the lower jaw, by the effort of the will, to the extent of about half the width of the finger. I advised her to continue the motion of the jaw, from time to time, as much as the soreness at the angle of the mouth would permit.

12th.—Some little tumefaction under the eye, but she makes no complaint. Directed an emollient poultice to the hard dressings upon the wounds on the neck.

14th.—Felt great comfort from the removal of the stiff dressings yesterday. The swelling of the cheek has subsided. Changed the lint again to-day.

15th.—She makes no complaint of her face, which in all respects is very promising. I removed three of the stitches from around the angle of the mouth, and re-applied lint and plaisters.

18th.—Every part of the flap appears to have united. Dressed the wounds with dry lint. Advised her to move the lower jaw a little downward every day several times.

May 12th.—Patch in the cheek entirely healed, (see fig. 2) The wound in the neck is nearly closed. She can open the jaws sufficiently wide to admit solid food.

Although I have before often operated for immobility of the jaw, and with the happiest results, and have once removed a deformity occasioned by a hole in the cheek, by the Taliacotian method, in the New York Hospital, with perfect success, I have never before seen it necessary to combine both operations in one individual; and the gratifying termination of this long and arduous case is a convincing proof of the excellence of the means employed, and of the power of art in repairing the ravages of disease, and in relieving its distressing consequences.

P.S.—*July.* A letter was received from Miss P. by the lady with whom she resided here, in which she stated her health to be excellent, the jaw to have acquired increased motion, and her friends to be much pleased with her improved appearance*.

SURGICAL CASES,

By George Bennett, M.R.C.S. &c. &c.

Case of Scrofula, illustrating the Efficacy of Sea Water in healing the Ulcerations.

Scrofula is considered to be of here-

* Condensed from the American Journal of the Medical Sciences. Nov. 1831.

ditary origin, but several occult causes also produce the disease, being found in individuals the families of whom have never, for generations, exhibited any symptoms of the disease. The case I am about to relate was an instance of this kind, the patient seeming the solitary individual of his family marked out for the disease.

Though generally an hereditary, it is by no means a contagious disease, and is often, perhaps, dependent upon a peculiar diathesis. Dr. Mason Good observes, that "he had a gentleman under his care who suffered from this disease, yet, of eight brothers and sisters who have reached the middle of life, he is the only one who has discovered any tendency to such a complaint; nor is it to be traced through any part of the family lineage, as far as it can be ascended."

Scrofula is a disease of weak vascular action, and its causes are various. Heat, in those constitutions unaccustomed to a tropical climate, might cause a languid action; excessive cold might have the same effect, as well as vicissitudes of temperature; but, in all instances, much must be attributed to individual diathesis.

When a primary affection, the disease is said to attack individuals of any temperament or habit of body; but when hereditary, "it most commonly," observes Dr. Cullen, "affects children of soft and flaccid flesh, of fair hair, and blue eyes, smooth skins and rosy cheeks; and such children have frequently a tumid upper lip, with a chop in the middle of it, and this tumor is often considerable, and extending to the columna nasi and lower part of the nostrils."

Mr. John C——, æt. 19, and midshipman on board the ship Sophia, came under my care, for glandular swellings on the neck, on the 23d July, 1830. This was his first voyage to sea, and also his first visit to a tropical climate. Previously to leaving England he had had no glandular swelling, and his health had been tolerably good. Since his arrival in tropical latitudes, he had been subject to eruptions of acne, and had had frequent attacks of diarrhœa. He is stout and thick set, of a fair and ruddy complexion, light hair, and blue eyes. He had had, for some time before his application to me, an extensive swelling on the right side of the face, especially of the parotid and submaxillary glands, which increased very slowly. He supposes it to have been produced by sleeping under an open port, in a current of air, about three months since; the day after which he felt a stiffness of the neck, and soon afterwards noticed the glandular swellings.

August 1st.—Pain and swelling slightly relieved by fomentations and a blister. Blister to be repeated, and a purgative powder administered.

3d.—Swelling on the right side reduced, glands on the left side slightly tumefied.

7th.—Glands on the left side have enlarged considerably; the chin feels numb from pressure on the nerves.

12th.—The submaxillary glands of the right side are much enlarged, one of which extends far round the back part of the neck.

23d.—I opened one of the glands on the left side, which had suppurated, and let out a quantity of curdy or cheese-like matter. Poultices were applied, and an alterative treatment adopted. Several other glands suppurated, and the ulcerations from them remained languid and indisposed to heal, under the application of digestive ointments.

October 16th.—Sea water to be applied two or three times a-day.

24th.—Ulcers are looking well.

From this time the ulcers gradually, but slowly healed, the sea water appearing to produce an healthy action. Some of the glands appeared reduced by the application, as they did not proceed to suppuration.

This case was cured by the 10th of January 1831, and the patient has had no scrofulous symptom since that time*.

Case of immediate Death from Rupture of the Heart.

On July 18th, 1827, a young man, named Adams, aged 27, residing at Plymouth, met with his death by the receding of a cart, which crushed him

* There is a curious circumstance respecting this patient, that for some time (upwards of three years) he had been subject to the hordeolum, or stye, which would be almost continually forming and suppurating; it constantly troubled him until, on the healing of the scrofulous ulcers, it also disappeared.

—a practice whereby, for increasing the revenue of government, the health and prosperity of myriads of individuals are destroyed, and whole families fall inevitable victims to disease and infamy.

I need only suggest what to every surgeon cannot but be obvious, namely, that rest alone, in an horizontal posture, with the application of linen spread with any mild unctuous substance, will generally be productive of a perfect cure. But in very few cases can that simple plan be adopted by persons whose unfortunate lot it is to endure these diseases, together with a train of others incident to their poverty. When a surgeon is consulted, he will find in frequent instances that something irritating has been applied to the wound; he will perceive the surrounding skin inflamed, and the discharge thin and fœtid; the ulceration gradually extending; the limb much swollen, and suffering from its own weight. All these symptoms are accompanied with continual pain, which increases during the night, in consequence of the exercise of the preceding day. In this state most surgeons would deem it indispensible to foment and poultice the part: to this there may be no objection, but I have seldom found it requisite. The first grand measure is, to attack and subdue the irritation and pain, by giving from two to five grains of opium daily, and about one grain of calomel night and morning, or every night, according as the constitution requires. As soon as the tenderness is diminished, let the dry suture be applied in the following manner:—

Having first defended the cicatrizing edges of the wound, and the surrounding inflamed and delicate integuments with a strip of lint or fine linen, spread with spermaceti ointment, palm oil, or any such mild preparation, by placing it on each side, the length of the wound; then apply a piece of adhesive plaister, cut tapering, about an inch in width at the broad end and two-thirds of an inch at the narrow end, and from eight to ten inches in length; the broad ends first, on the sound and uninflamed skin, beyond the lint, on each side of the ulcer; then draw the narrow ends as tight as can be comfortably borne, so as to approximate as much as possible the edges of the wound. In doing this the strips of plaister will cross each other, and afford the strongest support to the wound and the parts surrounding it. Two or three pairs of these strips will of course be necessary to cover and support an extensive wound. Lastly, apply the bandage, which is the most important part of the operation. This should be done, so as to afford the due support without creating uneasiness.

I am, Sir,

Your obedient servant,

H. Okes Bradford, M.R.C S.

216, Tottenham-Court-Road.

IDENTITY OF SMALL-POX AND COW-POX.

To the Editor of the London Medical Gazette.

Sir,

If you can spare some corner of your valuable Gazette for the following remarks, I request you will insert them.

I am, Sir,

Your Constant Reader.

Jan. 13th, 1832.

On reading in your last number, (Jan. 7th) Dr. Gregory's paper on the identity of small and cow-pox, I could not suppress a wish that experiments better calculated to settle the matter than what we learn respecting Dr. Sonderland's, might be instituted in this country, by attaching some cows, unfit for any thing else, to the vaccine establishment, and trying to communicate to them natural small-pox by inoculation. Should it succeed, a supply of lymph would be secured far superior to that obtained from human individuals at the risk of rendering vaccination imperfect, by disturbing the pustule. Cows might then, likewise, be kept at some of the hospitals in the country, and thus depôts for genuine lymph be multiplied.

If, as some think, vaccination is on the wane, it may be attributed to the virus, in its present state, having lost much of its protecting virtue, by passing through so many individuals, and thus becoming more and more assimilated to the human constitution.

The powerful and singular effects of transfusion of blood from one animal to another of a different species, are known. Perhaps a transfusion of morbid matter may produce somewhat analogous effects, and undergoing the anomalous

affection consequent on such a transfer from the cow, may render man insusceptible of the more ordinary form of the disease, generated in his own species. If so, assimilation by continued transmigration cannot but weaken the prophylactic power of the vaccine virus.

In reference to Dr. Burke's question, at page 505 of the same number, as to the modus operandi of the oil of turpentine in the case he communicates, it is evident that the inflammation of the eyes was metastatic, and having come on as soon as the irritation in the urinary organs had ceased, was again transferred to those parts on irritation being reproduced there by the judicious employment of the oil, which here had a counter-irritant, while in the other eye the calomel and opium had a sedative effect.

DR. STEVENS'S REJOINDER TO DR. JOHNSON.

To the Editor of the London Medical Gazette.

SIR,

I OBSERVE by your last number that you are unwilling to lend your pages to a controversy which has now become personal, but as you have already published another attack upon me, I hope that in common fairness you will also insert my reply.

It is painful for me to be compelled to appear so often before the public, on purpose to resist the unfounded attacks of an individual who is no sooner fairly convicted of one misrepresentation, than he tries to evade it by inventing another.

The chief accusation contained in Dr. Johnson's last letter, is one which concerns himself fully as much as it does me; for in it he asserts that the *analysis* of my paper on the Blood, which appeared in his journal for July 1830, was written by myself. Now there is but one charitable construction which I can put on this, and that is, to consider it as a modest intimation to the public, that any author who wishes to review his own works will find a place for the same in the Medico Chirurgical Review,—of which this accommodating individual is the sole editor.

It is very natural that a certain class of authors might wish to be accommodated in this kind of way; and lately there has been but too much reason to fear that such accommodation is not uncommon. Be this, however, as it may, there can be no doubt that the editor who permits such conduct, is just as culpable as the author who wishes to be the writer of his own review. I am sorry to observe that Dr. Johnson, in the present instance, has brought an accusation of this kind against himself; but, in as far as relates to this instance I will stand his friend, and try to prove that he is not guilty.

He asserts that, as he had not heard my paper read at the College of Physicians, I had called upon him, and requested that he would insert a *report* or *analysis* of the said paper, written by myself. "This," says he, "I did publish, and this is the unfair statement by which he (Dr. S.) says I wilfully endeavoured to mislead the public." As Dr. Johnson has fortunately furnished us with a great variety of polite expressions, I will borrow one of his own, and aver that there is scarcely one iota of truth in all this. I was not present myself, neither did I know whether Dr. J. was or was not present when that paper was read; nor did I care. Dr. J.'s journal may be, like others of the day, a useful medium for disseminating original facts; but those will be sorry to have the Doctor himself on their side, who have either read his own works, or who know the light in which his opinions are considered by the *intelligent* part of the medical profession. It is true that I did call upon him,—but not for the purpose which he has specified.

Some of your readers may recollect that in 1814 I published an account of an operation that I had performed in 1812, in which I had tied the internal iliac artery. The patient died in 1822, and the parts were preserved. Dr. Van Brackle, who had been present at the operation, and who was also present at the examination of the body after death, wrote a minute account of the preparation, and sent it to his friend, Dr. Chapman, (the then editor of the Philadelphia Journal.) which was published in that work in one of the numbers for 1822. Believing that this would find its way into the English journals, I did not write any account of it myself, but I kept the preparation in my possession, and brought it to England with me, when I returned to this country in 1829. And the truth is, I considered the preparation at that period as of little value; for I thought the circumstance of my having tied that artery was universally admitted. But in this I was deceived; for in May 1830, in looking over Mr. Lawrence's lectures, which had then just appeared, I found that he had expressed a doubt about the correctness of the statement which I had published. In consequence of this I waited upon Mr. L., told him that I had the preparation in my possession, and expressed a wish that he would examine it as early as possible. The preparation was immediately sent to the museum of the College of Surgeons, where it was examined by Sir Astley

ed inserting the bill itself at length, because the changes it has already undergone render the original version already obsolete; but as soon as it has become part of the law of the land, we shall transcribe it into our pages, and hail with gratitude a statute which will recognize anatomy, and place it on the same footing as the other liberal sciences.

ACTION OF AMMONIA ON THE VACCINE.

M. Nauche, of Paris, has given an account of some recent experiments of his, with regard to the power of ammonia over the vaccine virus. 1. Upon vaccinating with a lancet charged with the virus, but which had been exposed for some seconds to the vapour of ammonia, no development took place. 2. Inoculating one arm of the same patient with vaccine exposed to ammonia, and the other with pure vaccine, not only did no development take place in the first arm, but that in the second proved very imperfect. 3. After vaccinating a child as perfectly as might be, and in a few minutes afterwards making some slight punctures or incisions with lancets which had been exposed to the vapour of ammonia, the power of the vaccine was so far injured as to render the development very incomplete. These facts, M. Nauche thinks, taken in conjunction with the well-known effects of ammonia in various poisonous and contagious complaints, would warrant a fair trial for the volatile alkali in cholera, both as a preservative and a means of cure.

WESTMINSTER MEDICAL SOCIETY.

Saturday, January 14, 1832.

Phrenology and Insanity.

A paper by Mr. F. Winslow was read on the application of Phrenology to the elucidation of Mental Disorders.

The author began by observing, that the value of knowledge altogether depends on its susceptibility of being applied to practical purposes. Many who object to the abstract doctrines of phrenological science are continually inquiring, to what utility do they lead? Mr. Winslow thinks that a sufficient answer is given to the question by the fact, that one of the most important classes of diseases with which medical men have to combat, admits of being rationally elucidated by the aid of phrenology. Until Dr. Gall's time the phenomena of mind used to be explained by metaphysicians, without any reference to matter: *he* it was who "established, on an immutable basis, a system of mental philosophy, which, for its beauty and simplicity, stands unrivalled." "Reasoning *à priori*," observed Mr. W. "it certainly does appear absurd, that any person can have the power to form an estimate of the capacity of an individual, by examining the external conformation of his cranium; but there are many principles proved beyond all doubt, which an ignorant person would pronounce to be preposterous and absurd. Tell a man unacquainted with the rapid improvements of medical science, that you had just been eating your breakfast of bread made from sawdust, would he not pronounce you to be mad for making such an assertion?" And the author went on to prove, that of such sort of ignorant persons are those, who pronounce phrenology to be "a monstrous piece of quackery!" Dr. Spurzheim was the first who gave the public a satisfactory theory of mental derangement. Until his time, the definitions of insanity were vague and contradictory. Medical writers have erred in considering insanity as a specific disease; whereas the phrenologists, after Dr. Spurzheim, hold that mental derangement is *only a symptom* which may be produced by a variety of totally dissimilar affections. There are two hundred diseases, according to nosologists, to which the human eye is subject; and it would be just as futile to attempt to include all the symptoms of these diseases in *one* definition, as to frame one definition inclusive of every affection of the brain giving rise to insanity. Medico-legal writers have attempted to draw a line of demarcation between insanity and those idiosyncrasies of thought and feeling which, in the eye of the law, do not amount to mental derangement; and hence many definitions have been suggested by Dr. Haslam and others. Mr. W. expressed himself dissatisfied with Dr. Haslam's suggestion, that the physician's mind should be the standard of the patient's sanity; and entered, at considerable length, into examples of hallucination of a remarkable kind, from Shakspeare's, Luther's, and Sir Walter Scott's works. The diseases of the brain were curiously exemplified by reference to Hamlet and King Lear; and extracts from Dr. Combe's work were read. Dr. Gooch and Dr. Conolly, D'Israeli and Hazlitt, were also cited; and

Mr. Winslow amply adduced instances of men, particularly physicians, "of one idea." From all the facts which he stated, the author was of opinion, that too much care could not be devoted by medical men to the ascertaining the healthy, as well as the deranged, manifestations of the individuals presented to them. Dr. Haslam's standard he thought absurd. "Insanity cannot be defined; it is ridiculous to expect that all the varieties and forms of mental derangement can be included in one definition." A medical man of enlarged understanding can, after an attentive examination of the present and past conduct of his patient, tell whether insanity does or does not belong to him. In medico-legal investigations, where we are called upon as witnesses regarding insauity. Mr. Winslow thinks that we are not obliged to decide on the abstract question of the disorder, but upon the degree of mental derangement; and the slightest departure from a healthy condition of the brain, giving rise to deranged manifestations, ought to be looked upon as insanity. But it would be more proper for the commissioners of lunacy, on these occasions, to ask the opinions of medical men as to the existence of insanity, accompanied with an inability on the part of the patient to control himself or his affairs. "A man," said Mr. Winslow, "may be insane, yet perfectly competent to go at large, and to will away his property." In conclusion, the paramount importance of a knowledge of phrenology to the medical student was once more set forth, and this passage from Dr. Thomas Brown, the metaphysician, was quoted, as applying to phrenologists *par excellence*:—"He is unquestionably the philosopher most worthy of the name, who unites to the most accurate knowledge of mind, the most accurate knowledge of all the physical objects amid which he is placed; who makes each science to each, reciprocally a source of additional illumination; and who learns, from both, the noblest of all the lessons which they can give—the knowledge and adoration of that divine Being, who has alike created and adapted to each other, with an order so harmonious, the universe of matter and the universe of thought."

ALDERSGATE-STREET SCHOOL.

To the Editor of the London Medical Gazette.

Sir,

I feel confident that, whilst one leading object of your publication is to communicate valuable professional information, another scarcely less important one is to protect the rights of students, and to enforce regularity and attention on the part of their instructors. It is with great pain that I feel myself compelled to direct attention, through your publication, to neglect on the part of one of the anatomical teachers in the Medical School in Aldersgate-Street. The latter half of the course of demonstrations has been given with the greatest irregularity; and this morning, when the students assembled expecting to hear a demonstration (after the lapse of several days without one), notice was given that Dr. Roe was in the country with a sick friend, and could not leave. Discontent, on account of such repeated irregularity, has arisen to so great a height, that, by inserting this communication, you will do no less a service to the students at large than to the interests of the establishment itself.—I am, sir,

Your obedient servant,

A Pupil.

Aldersgate-Street,
Thursday morning, Jan. 19, 1832.

[The above is authenticated by the writer, who has communicated his name. We think it to the interests of the gentlemen connected with the school, to let them have an opportunity of refuting the statement, if it be unfounded, or remedying the evil complained of, if our correspondent's account of the matter be correct.—Ed. Gaz.]

METEOROLOGICAL JOURNAL,

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Jan. 1832.	Thermometer.		Barometer.	
19	from 23 to 34		from 30·26 to 30 20	
20	23	34	30·11	Stat.
21	29	41	30 07	30·16
22	32	43	30·16	30·15
23	33	42	30·14	30·24
24	24	46	30·14	30·09
25	37	47	29·74	29·76

Wind variable, S.W. prevailing.

Except the 24th, and afternoon of the 25th, cloudy; rain in the morning of the 25th.

Rain fallen, ·1 of an inch.

Charles Henry Adams.

NOTICES.

The communications of Dr. Allsop, Dr. Burrows, Mr. Ainsworth, and Mr. Dixon, were received too late for the present No.

The papers of Mr. George, Mr. Smith, Dr. Todd, Mr. Ashwin, Mr. Bateman, Mr. Davison, Mr. Stafford, and Dr. Copland, are in hand.

W. Wilson, Printer, 57, Skinner-Street, London.

sembling the white of boiled eggs—coagulated *albumen*.

Now this deposit, as it grows, softens down, and, like scirrhus and a scrofulous tubercle, it generally softens first in the centre. When it softens down, cavities are formed; or perhaps we might say cavities become discernible; and these cavities then chiefly contain blood, and on washing this away you see filaments or shreds floating in the cavity. If a portion of it be near the surface, the skin grows discoloured and the tumor adheres to the subjacent parts, and increases; or if the tumor be situated within, the serous membrane above it grows thin till it gives way. So that you may have the tumor ulcerate through the surface, or you may have a serous membrane within give way.

When the tumor ulcerates, a fungus shoots forth, but it is not firm, like the fungus of scirrhus, but, on the contrary, it is soft, easily torn, and bleeds very much. It is irregular, and of a dark red colour. When the fungus is very small, it exactly resembles the red soft polypi which grow from a mucous membrane. It grows very rapidly, and pours forth a fœtid sanious fluid; and people sometimes die, not from the irritation of the tumor, but from hæmorrhage. I had a patient who died from hæmorrhage, occasioned by one of these fungi in the bladder. There was a fungus as large as a walnut in the interior of the bladder, which never gave him any pain, which produced no irritation to the constitution, but it bled in spite of every thing which could be done; and he discharged, not only bloody urine, but pure blood, and at last sank under it. The hæmorrhage from a fungus of this description is often very copious. Now and then portions will slough.

This is a disease which affects every part of the body. It will affect the testicle—and then it is called by some, *soft cancer of the testicle*—the breast, the eye—it is very common in the eye of children—the uterus, ovaria, spleen, pancreas, liver, urinary bladder, brain, mesentery, and bones. It is continually seen in the extremities, and the absorbent glands become contaminated as they do in the case of common cancer; and when the glands are affected, and you make a section, they disclose nearly the same appearances as the original tumor, but there is this difference, it is said, that they never send forth a fungus.

You frequently have in the neighbourhood smaller tubera, circumscribed, but without a capsule at all, varying from the size of a pea to that of a walnut, of a pale greyish colour, and firmer than the original tumor, but they are the same disease. This is a disease which commonly affects other organs secondarily, so that when persons have it in the breast it is very common to find them gradually become the subjects of cough and pulmonic disease, and, on opening them, you find similar tumors within. It is said that the organs which are affected secondarily are never affected primarily—it is so said. It is very common for many organs to become affected at once; and so great is the disposition to this disease, when there is any disposition at all, that Mr. Travers says he has never known a person survive four years in whom he had operated for the extirpation of the disease. The constitution becomes impaired; there is a cachectic look even earlier than in scirrhus. Usually the person becomes emaciated, but now and then there is hardly any irritation at all, and you will sometimes see a person die of this disease in the stomach without having suffered any pain at all, and frequently without any person having suspected the nature or even seat of the disease. With respect to the case I mentioned of the disease in the urinary bladder, the man, although he died from the hæmorrhage occasioned by it, never suffered the least pain. Even when the mass is very considerable, there is generally little or no pain, and sometimes there is little or no irritation of the constitution. The case differs, therefore, in another respect very materially from schirrus. Schirrus, I mentioned, is almost always attended with violent deep lancinating pain; whereas, in encephaloid disease, there is very little pain, frequently none, and sometimes there is very little irritation.

A tumor of this description is exceedingly soft; its external appearance is remarkably smooth and equal, and it gives you the idea of fluctuation, so that I have known surgeons of great experience deceived in cases of this nature. They have imagined it was a collection of fluid, and plunged a lancet into it. I have seen this mistake occur over and over again, from its extreme resemblance, with respect to the touch, to a tumor containing fluid. Frequently, for a length of time, the part will not be at all discoloured. It is soft and elastic, and it is these two circumstances that give you the idea of fluctuation. The tumor, when taken out, is generally more or less round, and as I stated before, it differs from scirrhus in occurring at an early period of life. You saw a kidney the other night, taken from a child six years of age, in which several portions of disease were of this nature. It seemed to be a mixture of various diseases—some portions were scrofula, some were of scirrhous hardness, and in other parts there was certainly fungus hæmatodes.

I do not know what gives the disposition to this disease. I am not aware of any external circumstances which cause persons to be more liable to it, nor do I know whether it is hereditary. It is very probable that the

disposition to it is hereditary, exactly like the disposition to scirrhus, but it is carefully to be remembered that this is a disease, not of pain, like scirrhus—and that it is a disease which affects the opposite period of life to that in which scirrhus particularly prevails. Some consider it a mixture of sorofula and cancer.

At the next lecture I shall proceed to consider melanosis, and so terminate the consideration of structural diseases.

CLINICAL LECTURES,

Delivered in the Dispensary of the London University,

BY

DR. ANTHONY TODD THOMSON.

LECTURE IX.—Jan. 13, 1832.

Pulmonary Consumption.

GENTLEMEN,—In the case of the woman, Emma Warwick, who presented herself on the 31st of December last, you have had an opportunity of seeing the opinion that Phthisis is contagious fully verified. The husband of this poor woman had been a patient of the Dispensary for many months, and is now either dying, or has died, in one of the hospitals. He was in the last stage of Pulmonary Consumption, expectorating purulent matter, worn down by hectic, and presenting every symptom characteristic of the closing scene of that intractable disease. The poverty of the family of this man prevented his wife from occupying another bed; and she soon became the victim of decided Phthisis. The poor woman stated, that soon after her husband began to spit matter, she felt her breathing shorter, and more hurried than usual, on any slight exertion, and accompanied with pain of the side passing through to the back; she had also had a short troublesome cough, which lately much increased. Her countenance was sallow, her tongue furred, and her bowels were irregular, but generally confined. She had occasionally felt sick after taking food, and had lost her appetite; her urine was high-coloured. She could give no account whether any of her brothers or sisters were ever affected in a similar manner; but affirmed that she had always enjoyed good health until her husband became so ill. The husband, since her application here, was removed to an hospital, and her disease is rapidly advancing to a close, constituting that variety of the malady which has been termed *galloping Consumption**. I have brought the case before you, in order to make a few comments on the treatment of the disease, which is followed in this Institution, and on the particular fact of its contagious property, still doubted by many physicians.

The first mention of the contagious nature of Consumption which deserves notice, is by Galen; for I do not place much reliance on the query of Aristotle, which is rather to be referred to some floating popular opinions of its contagious nature, than to his own knowledge of the fact. Galen expressly says, that it is dangerous to pass a whole day with the consumptive; and his opinion is supported by Ballonius, Riverius, Morton, Van Swieten, Withering, Darwin, Dr. Reid, and others. Dr. Beardsley mentions an instance where the disease passed from a European family in the West Indies, to the negroes on the estate; and so strongly were both Morgagni and Valsalva impressed with the contagious character of this malady, that they even considered it dangerous to dissect the body of a person who had died of it—an opinion which, notwithstanding the justly-celebrated names of its promulgators, I cannot help designating as extremely absurd. On the contrary, Cullen doubted its contagious character, although he did not expressly deny it; and the same idea of it was entertained by Castillani, Heberden, and Beddoes. Portal thought that it was not contagious, but might be communicated by the milk of a nurse. The College of Tuscany decided completely against its contagious nature. As in many other affairs, truth lies in the middle; and my own experience leads me to coincide with Hoffman, that it is contagious only where an hereditary predisposition exists. In the case before us, and in many others which I have seen, this hereditary predisposition is not easily traced; but when it has occurred in the middling and higher walks of life, I have had little difficulty in tracing it. Of the existence of this predisposition there can be no doubt; and, if the presence of incipient tubercles in the lungs can be regarded as a proof of it, many instances might be adduced in which these bodies have been discovered in the lungs of infants, and those of adults who have died of diseases not at all of a pulmonary nature, and who have never displayed any obvious tendency to Phthisis. Now, it is easy to imagine that such individuals, placed in the situation of husband or of wife, or even of nurse to a person in the last stage of Consumption, and weakened by watching, fatigue, and anxiety, should be very susceptible of having disease set up in these latent tubercles, by constantly inhaling the breath of the consumptive patient. On the other hand, I cannot conceive how the disease can be communicated to a person possessing sound lungs, when we have every reason for thinking that, whatever may be the exciting causes, the anterior existence of *nascent tubercles* is an essential ingredient in the formation of Consumption—cold, or whatsoever can produce

* Since this lecture was delivered, the poor woman has died, and her husband also.

inflammation, merely converting the latent into active disease.

If the previous existence of nascent tubercles, therefore, in all cases of Phthisis be admitted, there is no difficulty of explaining the manner in which the breath exhaled from the ulcerated lungs of a consumptive person may communicate the disease to any one in whose lungs nascent tubercles exist. We know that, in such individuals, mechanical irritants—for instance, the fine metallic dust produced in pointing needles, the dust arising from the dressing of flax and feathers, and from other sources—excite the disease by their effect on the nascent tubercles. There can be no doubt, also, that the air expired from ulcerated lungs may carry along with it irritating or contagious matters; indeed, we have an assurance of this by the impression which the fœtor of the breath makes on the organ of smelling. These effluvia, carried into the lungs of another person, whether absorbed or merely applied to the lining membrane of the bronchial tubes and cells, will readily excite inflammation in the neighbouring tubercles, and set up that train of symptoms which constitute Pulmonary Consumption. In this manner I am persuaded that the disease is communicated to the predisposed, who sleep with, and are in close attendance on, individuals in the last stage of Phthisis; and that it is not the consequence of inflammatory action commenced in healthy lungs by any exciting cause whatsoever; nor is it dependent on a scrofulous diathesis, although much resembling it, since we find that the progress of tubercles to ulceration is not the same as that of inflamed glands, or tumors enlarged from lymphatic obstructions.

The consumptive diathesis is certainly not identical with that of Scrofula; yet both may exist in the same person. Dr. Withering mentions that one of the frequent characteristics of a consumptive habit, is an unusual magnitude of the pupil; this is also not unfrequent in strumous individuals, where no symptoms of the advancement of Consumption can be traced, and no hereditary predisposition is known to exist.

As some of you, gentlemen, have probably never seen tubercles, either in their nascent or diseased state, it may be proper to describe to you what appearances they present. Nascent tubercles are small, granular, indurated substances, found in the interstitial or cellular texture of the lungs, and in other parts of the body. You will also find it to be true, although not easily explained, that the left side of the lungs always contain more of these bodies than the right side, and the superior than the inferior part of the lungs. At any period anterior to the appearance of phthisical symptoms, in which circumstances have afforded opportunities of examining the lungs of those predisposed to consumption, these tubercles appear extremely minute, of a greyish white colour, nearly but not wholly opaque, and not unlike the seeds of the millet. Soon after the cough commences, they acquire a slight degree of transparency, and often present the appearance of dark yellow points dispersed thorough them. By degrees they enlarge, until they exceed the size of common peas, when they soften in the interior, and additional tuberculous matter being deposited around them, many coalesce into one mass, which is either in part internally changed into imperfect pus, or that substance is formed within them, and, filling their centres, dilates them, until they give way, at several points, and the pus oozing out is discharged into some of the neighbouring bronchial tubes, and expectorated. The ulcerated cavities thus formed sometimes become lined with a pellicle, beneath which a semi-cartilaginous membrane extends, constituting a kind of continuous surface, with the internal coat of the bronchial tubes, into which they open. It has, with much probability, been supposed that the enlargement of tubercles, and the formation of pus in them, is generally the result of inflammation set up in the lungs; and this is rendered more probable by the thickening and induration of the adjacent cellular matter, caused by coagulable lymph, the usual effect of inflammation, being thrown into it; and also by the not unfrequent expectoration of calcareous matter, which can only result from inflammatory action, in those habits in which nascent tubercles exist. Whenever, therefore, the exciting causes of inflammation influence the lungs, a new action commences in the vessels surrounding and supplying the coverings of the tubercles, and this continuing, the tubercles run into one another, suppurate, and form vomicæ, which, on emptying themselves, leave a large ulcerated surface exposed to the action of the air at every inspiration; so that hectic ensues in the same manner as if large ulcerated surfaces on the exterior, in highly irritable habits, were left exposed to the action of the air.

These remarks, gentlemen, might lead you to conclude, that in my opinion cold or sudden alternations of temperature, such as generally operate in causing pneumonic inflammation, is the sole exciting cause of phthisis whenever nascent tubercles exist; but this is too hasty a conclusion, and I am induced, like those who find others impatient in argument, to say—nay, nay—hear me out.

Always keeping in view the predisposition to the disease, and thence the probable existence of nascent tubercles, in every case in which real Phthisis occurs, the exciting causes of pulmonary consumption may be arranged under two heads, *physical* and *mental*.

The physical causes appear to comprehend many varieties of agents. Thus, as I have

already stated, mechanical irritants finding access to the lungs, may set up inflammation in pre-existing tubercles, or it may be set up by acrimonious states of the secretions, the consequence of other diseases, such as Scrofula or Small-pox. Measles, Scarlatina, and similar affections, may produce the same effect, or it may follow exposure to sudden alternations of temperature, capable of producing catarrhal inflammation in the chest. It is not necessary, however, that the temperature should be very low to produce this effect: I have seen it induced by sudden exposure to the evening air, in the middle of summer, when the thermometer was standing at 65°. With respect to the influence of *mind*, as an exciting cause of consumption, it is well known that persons predisposed to this disease are usually of a nervous temperament, highly susceptible of mental impressions. A look, a word, will hurry the action of the heart, even to palpitation; disturb and embarrass the respiration; influence powerfully the secretion of the liver, and impede the functions of the stomach. In such a state of habit, it is easy to conceive in what manner moral and intellectual agents may develop diseased action in nascent tubercles. Nothing so forcibly illustrates this influence of mind over body, even in altering organic structure, than the progress of the disease named *Nostalgia*. In this affection, when the longing for home cannot be gratified, emaciation gradually creeps on, attended by cough, pain of the chest, hectic, and many symptoms resembling those of pure Phthisis, until the patient has his desire gratified, or he sinks a victim to the disease. Postmortem examinations have detected appearances of inflammation, adhesions, and even ulcerations of the lungs. In the same manner, when much debility, accompanied with great irritability, consequent on too rapid growth, occurs; or when, in those predisposed to Phthisis, there is much mental disquietude, anxiety, or depression of the spirits (particularly in young females in the middle and higher ranks of life), congestions take place in the larger vessels of the lungs; the capillaries soon afterwards become also affected; inflammation of a sub-acute kind is set up, and morbid action in nascent tubercles rapidly developed. It is lamentable to reflect how often from these causes society is robbed of many of its most amiable and highly-accomplished members.

I may take this opportunity of remarking, that, in my opinion, hæmoptysis, which is put down by many authors as an exciting cause of consumption, is the result of such a state of lungs; and therefore it is to be regarded rather as an indication of a state of the chest likely to induce tubercular consumption, than as a remote cause of the disease. In these cases, there is no rupture of vessels; and the hæmorrhage is to be regarded as symptomatic of advancing tubercles in the lungs. I am aware that the high authority of Andral opposes this opinion, but my own observations are the source of my belief on this point.

To return to the consideration of mental anxiety as an exciting cause of Phthisis. Why, you may reasonably demand of me, do you not refer Emma Warwick's case to that source, rather than to contagion? My reply is, that I had not observed those sympathetic feelings in this woman that would lead me to form such a conclusion; and that the attack was too sudden also, to have originated in such a source. It would be unjust to affirm that she had not been anxious on her husband's account; at the same time her anxiety was not of that corroding kind which is likely to bring on such a rapid Consumption as she is now labouring under.

Gentlemen, in examining the chests of phthisical patients in this Institution, you have seen the ear, with and without the stethoscope, and percussion, employed; and it is proper that you should know what information you may expect from the employment of these tests of the state of the lungs. In the early stages of the disease, neither auscultation nor percussion are of much value; the tubercles are then rarely congregated in such numbers as to form a mass capable of so far obstructing respiration, in any portion of the lungs, so as to render it inaudible; or to afford a dull sound on percussion. But as soon as the expectoration assumes traces of a purulent character, then we may expect to discover pectoriloquism, more or less complete, indicating the existence of a cavity in the substance of the lungs. This is best ascertained by applying the excavated end of the stethoscope on various parts of the chest, and desiring the patient to utter a few syllables. If the voice of the speaker appear to proceed through the bore of the instrument, we may certainly conclude that there is a cavity of the lungs beneath it, communicating with the trachea by means of some of the bronchial branches. On the examination of the chest of Warwick, pectoriloquism was perfectly evident; but it could not be traced in that of his wife when she was examined, two days after she applied here for advice. In making this examination you should be aware, that in thin people with a sharp voice, some degree of pectoriloquism may be obtained by applying the instrument over particular parts of the chest, under the arm-pit, for instance, or at the junction of the clavicle and sternum, even when the lungs are perfectly healthy. It may be said that as correct a diagnosis might be drawn from the appearance of the sputa, as by the aid of the stethoscope; but the difficulty of distinguishing the mucus of an inflamed surface from pus, is too great to permit much reliance to be placed on this mode

of deciding; and no chemical test has yet been discovered for accurately determining the character of pus. The best test is that proposed, I believe, by Dr. Young, which depends on the colours produced by placing some of the expectorated matter between two pieces of plate glass, holding it near the eye, and looking through it at a distant candle. If the sputa be purulent, a circular arena of colours, green and red, will be observed surrounding a red area, of which the candle is the centre; but various circumstances may occur to render this test doubtful; it is, however, a good accessary to the stethoscope.

With regard to the treatment of Phthisis in its early stage, as few cases present themselves to *our* notice at that period, and having, therefore, little opportunity of exemplifying, by reference to cases, the correctness of my opinions, my remarks shall be very brief. As I have contended for the tubercular origin of the disease in all instances, you might, gentlemen, imagine that I must necessarily coincide with M. Bayle, and many other eminent men, in supposing that, owing to the natural tendency of tubercles to increase in size, soften, and suppurate, Consumption is incurable in its early stages; and that, if recovery ever take place, it must be, in a more advanced stage of the disease, the result of those curative efforts of nature, which form the semi-cartilaginous membrane on the surface of the tuberculous cavities, so as to constitute a kind of cicatrix, or shield, for guarding the diseased surface from the action of the air. But even admitting the accuracy of this opinion, are we to stand with our arms folded waiting the approach of this event, which is not likely to occur once in one thousand instances? Certainly not. It is our duty to arrest, if possible, the progress of the disease, admitting that we cannot cure it; and, therefore, those measures most likely to effect this, should instantly be resorted to. As yet the strength is not reduced; and bleeding, Cathartics, with other means of resolving inflammation, and low diet, should be employed. At first, nothing more will be requisite; for I am of opinion that Expectorants at this period are useless, and rather tend to do harm than to benefit the patient. Our principal object is to subdue the inflammatory action, and reduce the congested state of the pulmonary vessels; but the question is, whether this is to be accomplished by a full bleeding, or repeated smaller abstractions of blood. The degree of strength of the patient, the state of pulse, and the appearance of the blood, must regulate our decision. If there be evident febrile excitement, accompanied with pain on taking a deep inspiration, and a buffed state of the blood drawn with a full stream, then there can be no doubt of the propriety of repeating the venesection; but, if the pulse be soft and weak, although the blood may shew the buffy coat, it ought not to be repeated; this state of the blood being often the consequence of much nervous irritability, when no actual inflammation is present. In this case, however, the first bleeding may be useful in removing plethora; and although the lost volume of the blood is soon replaced, yet the abstraction, by increasing the *vis a tergo* of the overloaded vessels, enables them to contract upon their contents, and to carry forward the blood more freely. By this agent the tendency to morbid action in the capillaries, which would be the necessary result of the continuance of a congestion of the larger vessels of the lungs, is taken off. When a repetition of blood-letting is advisable, it is sometimes better to substitute cupping for the lancet. The chief mode, however, of reducing the inflammation in the early stage is by the influence of counter-irritants, or as they are termed, perhaps more justly, contra-stimulants. These may be divided into *internal* and *external*.

With respect to internal contrastimulants, the *Tartrate of Antimony and Potassa* is perhaps the best in this stage of the disease; and the effects which you have witnessed from its employment, both in incipient Phthisis and in chronic Bronchitis, in this institution, have been such as to secure your confidence in its contra stimulant powers. I have never found it necessary to prescribe it in the large doses recommended by the Italian physicians; and I have found that nausea is maintained better by small doses than by large. Even in doses of one grain every third or fourth hour, although nausea and vomiting follow the first and second dose, yet neither occur afterwards; whereas, by half-grain doses, I have been able to keep up a state of sickness for several days;—and it is only by doing so that any benefit is derived from its employment. The sudorific effect of the antimonial is not disadvantageous in this stage of the disease. Ipecacuanha has proved generally beneficial when given in combination with a neutral salt and some narcotic: perhaps the compound powder of Ipecacuanha of the Pharmacopœias, in conjunction with Tincture or Extract of Conium, is the best method of administering it. A slight nausea should be maintained, and the bowels at the same time kept only moderately open. Such a combination, given in conjunction with a drachm or more of distilled vinegar, and from one grain to five grains of Acetate of Lead, I have seen relieve hæmoptysis sooner than any other means. The vinegar prevents the deleterious influence of the salts of lead. *Tincture of Digitalis* is, in my opinion, prescribed in this stage of the disease on the most erroneous principles: it is not a direct sedative, but, like every other narcotic, its first action is stimulant—its second only sedative. When inflammation is present, whether in Dropsy

or in Phthisis, it is never useful; but it produces the most beneficial effects after the inflammatory action has been subdued by the aid of the lancet. If emetics be necessary, either Tartar Emetic or Ipecacuanha are preferable to more direct emetics, such as the sulphates of zinc or of copper; on account of the nausea induced by the former, and the influence which this exerts on the lymphatic and absorbent systems.

The most powerful external contrastimulents are blisters, the warm plaister of the Dublin Pharmacopœia, and the Tartar emetic ointment; and any one of them may be employed with advantage. If blisters be adopted, they should be frequently repeated, and applied alternately between the shoulders and on the fore part of the thorax. I have more than once used the actual cautery with benefit. The action of rubefacients is too transitory to prove permanently beneficial.

The diet of the Phthisical, in this stage of the disease, should be altogether milk, vegetables, and farinaceous matters. When milk oppresses the stomach, it may be diluted with soda water, which renders it more grateful, and aids the digestive powers of the stomach. One of the most important points with respect to regimen in this stage of the disease, is not available in that rank of society to which the patients of this and similar institutions belong; I mean removal, during winter, to a warm climate, or confinement to rooms graduated to a temperature not under 65 degrees. I knew a celebrated professor of the practice of medicine, whose wife, having a tendency to Phthisis, regularly disappeared with the swallows, and was not again visible in general society till late in the following spring; and by this means the disease was effectually warded off. In recommending, however, a graduate temperature, too little attention is paid to the humidity of the warm air: in every climate favourable to Phthisis, we find that the air is not only warm but moist. A hygrometer is as necessary an instrument in the winter conservatory of the Phthisical as a thermometer.

In the *latter* stage of the disease—that period of it at which most of the patients, who present themselves to us, have arrived—our hopes of cure, in my opinion, are futile; and consequently the object is to palliate—to restrain the frequency and violence of the cough—to improve the quality of the purulent secretion—to mitigate the hectic fever which wears down the patient, and to aid nature, if it be possible, in forming that membrane to which I have alluded, and which seems to be the only prospect of recovery, however rarely it is fulfilled. To attain the first indication, the Muriate of Morphia, in combination with the decoction of Cetraria Islandica, or other mucilages, has been found to answer extremely well; or, where opium in any of its forms cannot be employed, the Tincture of Conium, of the Dublin College, has been used with advantage. Nothing has appeared to improve the quality of the expectorated matter, and to promote its expulsion so effectually, as the inhalation of much diluted Chlorine. I have employed this agent for several years; and, besides being satisfied with its salutary influence in the cases in which it has been used, my confidence in it has been greatly confirmed by the accounts of its influence on the lungs of the workmen employed in the works of Mr. Tenant, of Glasgow, who is the greatest manufacturer of bleaching liquor, perhaps, in the world. Mr. Tenant informed me, that men who came to him with fixed coughs soon lost them when they were gradually brought into the chlorine house. In the cases which present themselves here, I generally order the Chlorine to be extricated from the common materials, a mixture of sea salt, black oxide of manganese, and sulphuric acid, in a corner of the sleeping apartment of the patient; and in all cases they have expressed, in strong language, the comfort it affords them; lessening the frequency of the cough, and greatly improving the quality of the expectorated matter. Nor is this at all difficult to be explained, if we revert to the beneficial influence which it exerts in its aqueous solution, as a gargle in malignant Scarlatina, and as a lotion in fœtid and ill-conditioned ulcers on the surface of the body.

With the view of mitigating hectic, I have had little reason to be satisfied with the effects of Foxglove, Myrrh, the Balsams, or tonics in any form, with the exception of vinegar, which I have given in doses of half a fluid ounce three times a-day, in combination with Opium and the decoction of Cetraria; whilst, at the same time, the trunk of the body has been sponged with vinegar and water before the patient gets out of bed in the morning. This plan seems to alleviate the hectic almost in the ratio of its influence, in moderately confining the bowels; and when its internal employment has disagreed with the stomach, I have found nearly equal advantage from the use of the Chalk mixture, or solutions of Catechu or Kino. In proportion as the diarrhœa has been moderated, or rather as the bowels have become confined, has amendment, or rather mitigation of the symptoms, taken place; and, instead of aggravating the febrile symptoms, this state of bowels has almost invariably appeared to alleviate them. The truth of this remark was strikingly verified in one of the cases which was lately before us.

not attribute it to his management, since it was occasioned by the *syncopal state of the animal*, which is known to every surgeon to be a hemostatique state. In short, in the three other experiments the animals died of hemorrhagy notwithstanding the use of *noix de galles*, of plain water, of Ruspini's liquor, of sutures, &c. &c. Mr. Hawkins concludes by saying, *that it is possible to stop the hemorrhagy of an arterial wound by pressure during the space of ten minutes*, as also by the application of small compresses, which may be dipped in a variety of liquids; but I will observe that it is not by such experiments as those above-mentioned that Mr. Hawkins can plausibly adopt such conclusions, for they would evidently be erroneous, when it is seen that out of five experiments that he has tried, not a single one of them turned out satisfactory, though he made use of the compresses he mentions, for he could not avoid having recourse to the suture seams in every case that he attempted. We know that *pressure*, a *coagulum*, &c. &c. can sometimes stop the hemorrhagies; but are there not cases in which such means cannot be made use of, and then is it not very painful to be in want of a remedy absolutely hemostatique, when all other means, as yet known prove fruitless? Mr. Hawkins afterwards describes the effects produced by stypties in the stoppage of the blood; but here again he quite mistakes when he includes our *liquid hemostatique* in the number of stypties. Our liquid is not styptic, for it is not astringent; but, on the contrary, is *hemostatique;* for, as it is very justly acknowledged by Mr. Hawkins, it possesses the virtue of coagulating the blood to such an unequalled degree, that the surgeon who uses it has only to apply an imbibed tampoon, or compress, to the wound, and it stops the hemorrhagy immediately, without even subjecting the practitioner to stain his fingers with the gush of blood that generally occurs in all other modes of proceeding.

Mr. Hawkins is perfectly right when he says *that the action of the styptic seems only a secondary one to that of the pressure on the parts to stop the hemorrhagy*. And again, I repeat it, our liquid is neither *styptic* nor *astringent*, but its virtue is that of coagulating the blood; and as to its nature, it is perfectly single, and bears no sort of similarity whatever to any preparation that may be known to Mr. Hawkins or to any other persons; and as to its efficacy, we have good reasons to believe that its virtues are far more extensive when applied to the human body than to that of a sheep, which is the animal that we fixed upon to try our experiment, as being of a nature to live on grass, and having a poorer and more limpid blood than those animals that live on flesh; therefore had the experiment been tried on a man, it is of all probability that the desired effect would have proved instantaneous and radical.

We will now beg leave to lay before the reader an exact statement of the experiments and results that we performed and obtained in Paris. We will proceed to do it without giving the names of the many distinguished surgeons that were present; being, therefore, certain of our success, and of the good faith and frankness of what we set forward to so enlightened a society as that of London, we have not the least fear of being contradicted either by our fellow-practitioners in Paris or by those not less eminent in London.

The incisions in the artery were longways, and described as follows:—

Nature of the Incisions.		Lived.	Died.
Longways	11	11	0
Transverse	15	15	0
Deperdition or loss of substance	8	5	3
Pricked	1	1	0
Amputation	1	1	0
	36	33	3

It will be observed that we several times opened the carotid artery of one of the sheep thus operated on, and we performed an amputation on its thigh, which the animal survived, and four months and a half elapsed before we killed it to extract the artery, which we still have in our possession. I now come to the letter addressed to you by Mr. Hawkins, and published in your Medical Gazette of the 11th of February, 1832; and we will say for ourselves that we are by no means ignorant of the experiments of Dr. Jones, as Mr. Hawkins seems to think it, for

we agree that these experiments, on some points, have thrown a degree of light, particularly concerning physiology and pathology; but relative to surgery, I will ask Mr. Hawkins who is the surgeon that would have recourse to the use of an external suture seam on the artery, to stop the hemorrhage? Has Mr. Hawkins ever performed the like on a man? Should he have done it, or have an opportunity of doing it, we would feel very grateful to him for his notice to allow us to witness it.

He now says that there are many persons who ought to know more of the prineiples of treating and stopping hemorrhagy, who have been deceived by being induced to place confidence in the *soit disant or supposed efficacy of Messrs. Talrich and Halma-Grand's styptics, &c.* We will merely reply by this plain and plausible argument—why did not Mr. Hawkins's method of operating prove satisfactory in the five experiments that he performed, and of which *only one* seemed to exhibit some kind of sueeess? We will, however, not enter any further on the subjeet of Mr. Hawkins's *rather too direct insinuations* of our having attempted to speculate on the eredulity of a publie well known for its encouragement and lenity to foreigners and their productions. How could we ever have come forward with such views (admitting even that we had no regard for prineiple or reputation), when all our demonstrations and experiments have been performed, and are still offered to be performed, publicly; in presence of a scientifie body of praetitioners, and, in faet, in presence of any one that may appear to us capable of valuing the advantages and relief that our discovery offers? Therefore we shall drop the argument, and wait the result that will douhtless by and by constitute the opinion of the high-minded and impartial society in the midst of which we stand, and to which we confidently appeal; observing again that our liquid hemostatique *is not of an astringing but of a coagulating nature, that clots the blood without any incorporation whatsoever.* We will therefore eonclude by saying, that we hope to find so distinguished a man as Mr. Hawkins more impartial in future, partieularly if he would have the kindness to use his powerful influenee to assist us in procuring opportunities to further the performanee of our experiments, which, no doubt, would convince him that our *liquid hemostatique* is quite a new thing; that its power is positive, its virtue very great, and, in fact, a very beneficial discovery for humanity; which motives, we trust, will be justly appreciated by so eminent a professor as Mr. Hawkins. We will now lastly say, that we will always he found ready to witness or perform any experiment that may be suitably requested of us; adding, at the same time, that all our proceedings will be guided by *honour*, *integrity*, and *energy*, feeling ourselves perfeetly entitled to assume the title of medical men; therefore we will not be found to draw back from any circumstance or seientific discussion whatsoever that we may be offered to participate in or originate. Should this appeal to our fellow physicians of Great Britain in general, but more particularly to those residing in London, remain unnoticed, we would consider it a very unfriendly act, and as a proof of their inability to eo-operate with us in the search or practice of such seientific discovery as ennobles the mind and assists humanity. Meanwhile, I have the honour to remain,

Sir,

Your most obedient
humble servant,
HALMA-GRAND, M.D.

Professor of Anatomy to the University of Paris.

32, St. James's Street, Piccadilly.

[We have given insertion to the above, chiefly because it is intended as an answer to the paper by Mr. Hawkins which appeared in this journal. Dr. Halma-Grand is probably not aware of the discreditable light in which, in this country, any one professing to have *secret* remedies must be content to stand; and we may add that had he not been a foreigner the circumstance alluded to would have prevented us from giving a place to his letter.—E. G.]

SMALL-POX AFTER VACCINATION.

To the Editor of the London Medical Gazette.

SIR,

In some remarks upon vaccination, by Dr. Howison, in your number for the

14th January, there is a letter from Dr. Sanders to Dr. H., in which Dr. S. maintains "that the real small-pox never occurred, either in its complete form, or under any modification, in a person who had undergone vaccination." I am aware of the great difficulty of deciding this point. It must, however, be allowed that authors of great eminence have stated their belief in, and even given instances of this occurrence. Yet Dr. Sanders tells us "that in the writings of the anti-vaccinists, as well as in those who believe in the occasional failure of vaccination, there is not one instance of the small-pox." In his quotation from the *Dictionnaire des Sciences Médicales*, I presume he has written *varicella* for *variola*. Will the following case, which I watched with peculiar care, and now extract from my note-book, for the first time, afford him any satisfaction?

——— aged 22, when a child, was vaccinated at the dispensary, and from strict inquiry, seems to have undergone the disease very favourably. The marks still remain very large and distinct, one on each arm. On the 20th May, 1831, was seized with febrile symptoms, accompanied with vomiting and headache.

21st.—Fever more considerable, face flushed, tongue foul, eyes red, much thirst.

22d.—An eruption appeared this morning, first on the face, then on the neck, arms, and chest. In the evening it extended over the trunk and part of the thighs and legs. Eruption resembles small inflamed acuminated pimples.

23d, 2d of eruption.—The entire body now covered, and the eruption is very thick on the under extremities and parts liable to an accumulation of heat, as in the groins. Very thick on the face. Pimples of yesterday have a whitish clear small vesicle at top.

24th, 3d of eruption.—Pustules still thicker over the body. Those of yesterday more opaque, white fluid at top, surface plain. One or two, which have been rubbed during the night, broad, depressed, and out of all character, especially two on the left thigh, one of which shews, as it were, a large perforation in the centre.

25th, 4th of eruption.—Pustules of a dirty pearly whiteness to within one-third of the base; plain at top; suppuration distinctly marked.

26th, 5th of eruption.—Pustules still flatter at top, and several of them coalesced; those on the face very much so, forming a thickish yellowish green exudation, or soft crust.

27th, 6th of eruption.—Pustules of the face more coalesced, and thin crusts forming.

28th, 7th of eruption.—Vesicles on the body becoming turbid; face somewhat swollen; throat sore, and considerable difficulty of swallowing; eyelids shut up.

29th, 8th of eruption.—All the symptoms regularly progressing; inside of the mouth very red and sore; sleep much disturbed.

30th, 9th of eruption.—Restlessness much relieved by opiate.

31st, 10th of eruption.—Feet beginning to swell; pustules here of a very dark colour; throat better; secretion of the mouth becoming very thick and viscid.

June 1st, 11th of eruption.—Feet very sore, and considerably swollen; pustules of the soles of the feet have a bruised appearance; feverishness, which had much subsided, again returning.

From this time the disease went on in a regular form, every symptom being minutely compared with what generally occurs in common small-pox; but, merely to look at the patient was enough to convince any one of the genuineness of the disease. You will observe, also, that the above was not a very mild case, the pustules being on many parts confluent, and the secondary fever severe. I am also persuaded that had it not been for the previous vaccination, the disease might have proved fatal, as it shewed more than one symptom of a malignant tendency.

What I understand by a modified form of small-pox, in contradistinction to Dr. Sanders, is not that it is a new disease, compounded of something between cow-pox and small-pox, but the genuine small-pox in a mild or softened form, and this is corroborated by Dr. Sonderland's view of the identity of the two diseases.—I am, sir,

Your most obedient servant,
JAMES RANKINE, M.D.

Hanley, Feb. 6th, 1832.

PAPERS AND CORRESPONDENCE

ON

CHOLERA.

OPINION OF HUFELAND

ON THE

ORIGIN AND PROPAGATION OF CHOLERA*.

THE cholera is originally the product of atmospherico-terrestrial re-action; but it also, in its highest degree of development, generates a contagion which may be communicated from person to person. It thus unites miasma and contagion, aerial and human infection. It is propagated from place to place in two ways; the one by progressive atmospheric infection, which particularly follows the course of rivers, as was recently shewn in the communication of the disease to Berlin, which did not take place overland, but by the water course of the Warte, the Finnow Canal, and the Havel; the other by infected persons and things, whereby it is especially to be observed, that the infection is extremely conditional, and therefore rare, very few patients attaining the height of the infective power of the disease, and very few persons possessing the requisite susceptibility for taking the same.

To me it is, in fact, inconceivable, how among physicians there can be any dispute on this point, or how they should, as in some instances, have been divided into two parties, contagionists and anti-contagionists, standing in hostile opposition to each other. Is it not a long-recognized truth, that a disease may originate from epidemic influences and then develop a contagion, and that the same disease may at the same time be produced by the atmosphere, and by a contagion? No one doubts that catarrh may be epidemically produced, by generally-prevailing damp cold air, and as little that an individual labouring under the highest degree of catarrh may, by a kiss, impart it to another. Is not this equally the case with scarlet fever, with dysentery, and with hooping-cough, &c.? Taking into consideration all that experience has hitherto proved in Prussia, and here in Berlin, the whole may be summed up in the few following conclusions.

First, The cholera may be received either from the air, or by infection.

Secondly, The first mode of communication is much more frequent than the last.

Thirdly, To take it in either way, a peculiar susceptibility of constitution is requisite.

Fourthly, This susceptibility may be produced, or favoured, by certain influences which are under our control; these are, overloading the stomach; the use of sour, flatulent, fermenting, cold, indigestible food and drink; over-indulgence in spirituous liquors; taking cold, getting wet, residence in damp air, and depressing emotions. Experience has, in innumerable instances, shewn that immediately after the operation of such powers, (*potenzen*) cholera has shewn itself; while, on the contrary, no example exists in which, without such previous operation, cholera has taken place.

Fifthly, The prevention of cholera, by keeping off the infectious matter, is only partially possible, as the communication thereof through the atmosphere cannot be prevented at all, and that through personal contagion, can only be prevented in part. Still, the measures adopted by Government are laudable, and deserving of our thanks.

Lastly, The most effectual method, therefore, of protection against the disease is, preventing the susceptibility thereto by avoiding those causes which favour it.

Berlin, Dec. 1831.

* Translated from the original German MS. in our possession.—E.G.

PROPER MODE OF EMPLOYING GALVANISM IN CHOLERA.

To the Editor of the London Medical Gazette.

March 5, 1832.

SIR,

IN the last number of the Medical Gazette two cases of cholera are mentioned, in which galvanism was employed without benefit. My attention from various circumstances having been directed to the employment of galvanism